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AND

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BY WILLIAM NICHOLSON.

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PREFACE.

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The Engravings consist of 1. Figures 1, 4, 5, 6, and 7, (in one Quarto Plate) of Mr. Troughton's Apparatus for dividing Astronomical Instruments; 2. Figs. 2 and 3, of Mr. Troughton's Apparatus for dividing Astronomical Instruments; 3. Diagrams illustrating the Art of Aerial Navigation, by Sir George Cayley, Bart; 4. Leaf of the Mimosa Pudica, by Mr. Robert Lyall; 5. Sir George Cayley on Aerial Navigation; 6. Apparatus for procuring Potassium; 7. Dr. Wollaston's reflecting Goniometer; 8. The Intervertebral Joint of the Shark; 9. Mr. Miller's Drag for finding and raising the Body of a Person drowned; 10. His Reel Safeguard, for the Security of a Person going to the Assistance of one Drowning; 11. His Missile Rope to be flung to a Person in Danger of being drowned; 12. Apparatus of Messrs. Allen and Pepys, for the Respiration of Gasses by Animals; 13. Lieutenant Spratt's Homograph; 14. Mr. White's improved File for Letters, Receipts, &c.; 15. Diagrams illustrative of certain Properties of Solids, by John Gough, Esq.; 16. Capt. W. Bolton's Method of rigging his Jury Mast, and setting up a Ship's lower Rigging.

TABLE

TABLE. OF CONTENTS

TO THIS TWENTY-FIFTH VOLUME.

JANUARY, 1810.

FEBRUARY,

CONTENTS.

FEBRUARY, 1810.

Engravings of the following Subjects: 1. Figs. 2 and 3, of Mr. Troughton's Apparatus for dividing Astronomical Instruments: 2. Diagrams illustrating the Art of Aerial Navigation; by Sir Geo. Cayley, Bart. 3. Leaf of the Mimosa Pudica; by Mr. Robert Lyall.

MARCH.

MARCH, 1810.

APRIL,

APRIL, 1810.

SUPPLEMENT.

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ARTICLE I.

An Account of a Method of dividing Astronomical and other Instruments, by ocular Inspection; in which the usual Tools for graduating are not employed; the whole Operation being so contrived, that no Errour can occur but what is chargeable to Vision, when assisted by the best optical Means of viewing and measuring minute Quantities. By Mr. EDWARD TROUGHTON. *Communicated by the Astronomer Royal*[*].

Division of astronomical instruments.

IT would ill become me, in addressing myself to the members of this society upon a subject which they are so well enabled to appreciate, to arrogate to myself more than may be assigned as my due, for whatever of success may have been the result of my long continued endeavours, exerted in prosecuting towards perfection *the dividing of Instruments immediately subservient to the purposes of astronomy*. A man very naturally will set a value upon a thing,

[*] Philos. Trans. for 1809, p. 105.

 on

on which so much of his life has been expended; and I shall readily, therefore, be pardoned for saying, that con-[illegible] some attainments which I have made on this sub-[illegible] too valuable to be lost, and being encouraged also by the degree of attention which the Royal Society has ever paid to practical subjects, I feel myself ambitious of presenting them to the public through what I deem the most respectable channel in the world.

The author's attention early turned to it.

It was as early as the year 1775, being then apprentice to my brother, the late Mr. John Troughton, that the art of dividing had become interesting to me; the study of astronomy was also new and fascinating; and I then formed the resolution to aim at the nicer parts of my profession.

His brother skilful in it.

At the time alluded to, my brother, in the art of dividing, was justly considered the rival of Ramsden; but he was then almost unknown beyond the narrow circle of the mathematical and optical instrument makers; for whom he was chiefly occupied in the division, by hand, of small astronomical quadrants, and Hadley's sextants of large radius. Notwithstanding my own employment at that time was of a much inferior nature, yet I closely inspected his work, and tried at leisure hours on waste materials to imitate it. With as steady a hand, and as good an eye, as young men generally have, I was much disappointed at finding, that after having made two points, neat and small to my liking, I could not bisect the distance between them, without enlarging, displacing, or deforming them with the points of the compasses. This circumstance gave me an early dislike to the tools then in use; and occasioned me the more uneasiness, as I foresaw that it was an evil which no practice, care, or habit could entirely cure: beam-compasses, spring-dividers, and a scale of equal parts, in short, appeared to me little better than so many sources of mischief.

Defect of the common practice.

Turning supposed to be nearest perfection.

I had already acquired a good share of dexterity, as a general workman. Of the different branches of our art, that of *turning* alone seemed to me to border on perfection. This juvenile conceit, fallacious as I afterwards found it, furnished the first train of thoughts, which led to the method about to be described; for it occurred to me, that if I could by

by any means apply the principle of turning to the art of dividing instruments, the tools liable to objection might be dispensed with. The means of doing this were first suggested by seeing the action of the perambulator, or measuring wheel; the surface of the Earth presenting itself as the edge of the instrument to be divided, and the wheel of the perambulator as a narrow roller acting on that edge; and hence arose an idea, that some easy contrivance might be devised, for marking off the revolutions and parts of the roller upon the instrument. Since the year above-mentioned, several persons have proposed to me, as new, dividing by the roller, and I have been told, that it also occurred long ago to Hook, Sisson, and others; but, as Hutton on watch-making says, "I do not consider the man an inventor, who merely thinks of a thing; to be an inventor, in my opinion, he must act successfully upon the thought, so as to make it useful." I had no occasion, however, to have made an apology for acting upon a thought, which, unknown to me, had been previously conceived by others; for it will be seen in the sequel, how little the roller has to do in the result, and with what extreme caution it is found necessary to employ it.

Application of a roller to marking divisions.

When a roller is properly proportioned to the radius of the circle to be divided, and with its edge made a small matter conical, so that one side may be too great, and the other side too little, it may be adjusted so exactly, that it may be carried several times around the circle, without the errour of a single second; and it acts with so much steadiness, that it may not unaptly be considered as a wheel and pinion of indefinitely high numbers. Yet, such is the imperfection of the edges of the circle and roller, that, when worked with the greatest care, the intermediate parts, on a radius of two feet, will sometimes be unequal to the value of half a minute or more. After having found the terminating point of a quadrant or circle so permanent, although I was not prepared to expect perfect equality throughout, yet I was much mortified to find the errours so great, at least ten times as much as I expected; which fact indicated, beyond a doubt, that if the roller is to be trusted at all, it must only be trusted through a very short arc. Had there been any

When most accurate, defective.

Must be confined to short arcs.

thing slippery in the action, which would have been indicated by measuring the same part at different times differently, there would have been an end of it at once; but, this not being the case in any sensible degree, the roller becomes a useful auxiliary to fill up short intervals, the limits of which have been corrected by more certain means *.

Bird,

Porosity of the metal a source of errour.

The roller and metal to be divided yield to each other.

* There are two things in the foregoing account of the action of the roller, which have a tendency to excite surprise. The first is, that the roller should, in different parts of its journey round the circle, measure the latter so differently. One would not wonder, however, if in taking the measure across a ploughed field, it should be found different to a parallel measure taken upon a gravel walk; and, in my opinion, the cases are not very dissimilar. Porosity of the metal, in one part of the circle more than in the other, must evidently have the same effect; brass unhammered is always porous; and the part, which has felt the effect of two blows, cannot be so dense as other parts which have felt the effect of three; and, should the edge of the circle be indented by *jarring turning*, it would produce a visible similitude to ploughed ground. Every workman must be sufficiently upon his guard against such a palpable source of errour; yet perhaps with our greatest care we may not be able to avoid it altogether. The second is, that notwithstanding the inequality abovementioned, the roller having reached the point upon the circle from which it set out, should perform a second, third, &c., course of revolutions, without any sensible deviation from its former track; this is not perhaps so easily accounted for. It must be mentioned, that the exterior border of the circle should be *turned rounding*, presenting to the roller a convex edge, the radius of curvature of which is not greater than one tenth of an inch. Now, were the materials perfectly inelastic and impenetrable, the roller could only touch the circle in a *point*, and in passing round the circle, it could only occupy a *line* of contact. This in practice is not the case; the circle always marks the roller with a broad list, and thereby shows, that there is a yielding between them to a considerable amount. The breadth of this list is not less than one fiftieth of an inch; and it follows, that at least 12° of the circle's edge must be in contact at the same time; that the two surfaces yield to each other in depth, by a quantity equal to the *ver. sin.* of half that arc, or 1-1800th of an inch: and that the circle has always hold of the roller by nearly 1° of the edge of the latter. Whoever has examined the surfaces of metals, which have rolled against each other, must have observed that peculiar kind of indentation, that always accompanies their action; and there can be no doubt, that the particles of a roller, and those of the surface on which it acts, which mutually indent each other, will, upon a second

course

Bird's method of division.

Bird, who enjoyed the undisputed reputation of being the most accurate divider of the age in which he lived, was the first who contrived the means how to render the usual divisions of the quadrant bisectional; which property, except his being unusually careful in avoiding the effects of unequal expansion from change of temperature, chiefly distinguished his method from others who divided by hand. This desirable object he accomplished by the use which he made of a finely divided scale of equal parts. The thing aimed at was, to obtain a point upon the arc at the highest *bisectional number of divisions* from 0, which in his eight feet quadrants was 1024, = 85° 20′. The extent of the beam compasses, with which he traced the arc upon the limb of the instrument to be divided, being set off upon that arc, gave the points 0° and 60°; which, being bisected, gave 30° more to complete the total arc. A second order of bisections gave points at 15° distance from each other; but that which denoted 75° was most useful. Now, from the known length of the radius, as measured upon the scale, the length of the chord of 10° 20′ was computed, taken off from the scale, and protracted from 75° forwards; and the chord of 4° 40′, being ascertained in the same manner, was set off from 90° backwards, meeting the chord of 10° 20′ in the continually bisectional arc of 85° 20′. This point being found, the work was carried on by bisections, and the chords, as they became small enough, were set off beyond this point, to supply the remainder of the quadrantal arc.

Mr. John Troughton's method of division.

My brother, whom I mentioned before, from mere want of a scale of equal parts upon which he could rely, contrived

course begun from the same point, indent each other deeper: this is not, however, exactly the case in question; for, whatever of fitting might have taken place between the surfaces of our roller and circle in the first revolution of the former, we should imagine would be obliterated by the fifteen turns which it must repeat over fresh ground. Experience shows, however, as every one will find who tries the experiment with good work, that on coming round to the point of commencement, the roller has the disposition to regain its former track; for, were this not the case, although the commensurate diameters were adjusted so exactly as to be without sensible errour in one course, yet a less errour than that which is so would become visible, when repeated through many courses.

the

viding bisectionally without a scale.

the means of dividing bisectionally without one. His method I will briefly state as follows, in the manner in which it would apply to dividing a mural quadrant. The arcs of 60° and 30° give the total arc as before; and let the last arc of 30° be bisected, also the last arc of 15°, and again the last arc of 7° 30′: the two marks next 90° will now be 82° 30′ and 86° 15′, consequently the point sought lies between them. Bisections will serve us no longer; but if we divide this space equally into three parts, the most forward of the two intermediate marks will give us 85°, and if we divide the portion of the arc between this mark and 86° 15′ also into three, the most backward of the two marks will denote 85° 25′. Lastly, if we divide any one of these last spaces into five, and set off one of these fifth parts backwards from 85° 25′, we shall have the desired point at 1024 divisions upon the arc from 0°. All the rest of the divisions which have been made in this operation, which I have called marks because they should be made as faint as possible, must be erased; for my brother would not suffer a mark to remain upon the arc to interfere with his future bisections.

Smeaton's preference of division by the computed chord.

Mr. Smeaton, in a paper to be more particularly noticed presently, justly remarks the want of a unity of principle in Mr. Bird's method; for he proceeds partly on the ground of the protracted radius, and partly upon that of the computed chord; which, as Smeaton observes, may or may not agree. Bird, without doubt, used the radius and its parts in order to secure an exact quadrant; but Smeaton, treating exactness in the total arc as of little value to astronomy, would, in order to secure the more essential property of equality of division, reject the radius altogether, and proceed entirely upon the simple principle of the computed chord.

Advantages of Mr. John Troughton's method.

The means pursued by my brother, to reach the point which terminates the great bisectional arc, is the only part in which it differs from Bird's method; and I think it is without prejudice, that I give it the preference. It is obvious, that it is as well calculated to procure equality of division, as the means suggested by Smeaton; at the same time that it is equal to Bird's in securing the precise measure of the total arc. It proceeds entirely upon the principle of the protracted

tracted chord of 60° and its subdivision; and the uncertainty, which is introduced into the work by the sparing use which is made of subdivision by 3 and 5, is, in my opinion, likely to be much exceeded by the errours of a divided scale*, and those of the hand and eye, in taking off the computed chords, and applying them to the arc of the instrument to be divided.

Ramsden's method by the engine.

Ramsden's well known method of dividing by the engine unites so much accuracy and facility, that a better can hardly be wished for: and I may venture to say, that it will never be superseded, in the divisions of instruments of *moderate radii.* It was well suited to the time in which it appeared; a time when the improvements made in nautical astronomy, and the growing commerce of our country, called for a number of reflecting instruments, which never could have been supplied, had it been necessary to have divided them by hand; however, as it only applies to small instruments, it hardly comes within the subject of this paper.

Hindley's method.

The method of Hindley, as described by Smeaton †, I will venture to predict will never be put in practice for dividing astronomical instruments, however applicable it might formerly have been for obtaining numbers for cutting clockwork, for which purpose it was originally intended. It consists of a train of violent operations with blunt tools, any one of which is sufficient to stretch the materials beyond, or press them within their natural state of rest; and, although the whole is done by contact, the nature of this contact is such, as, I think, ought rather to have been contrasted with, than represented as being similar to, the nature of the contact used in Smeaton's Pyrometer, which latter is performed by the most delicate touch; and is represented, I believe justly, to be sensible to the [illegible] part of an inch. Smeaton has, however, acquitted himself well, in describing and improving the method of his friend; and the world is particularly obliged to him for the historical

* That Bird's scale was not without considerable errours, will be shown towards the end of this paper.

† Phil. Trans. for 1788.

part

part of his paper, as it contains valuable information, which perhaps no one else could have written.

London practice of dividing large instruments.

The only method of dividing large instruments now practised in London, that I know of, beside my own, has not yet, I believe, been made public. It consists in dividing by hand with beam compasses and spring dividers, in the usual way; with the addition of examining the work by microscopes, and correcting it, as it proceeds, by pressing forwards or backwards by hand, with a fine conical point, those dots which appear erroneous; and thus adjusting them to their proper places. The method admits of considerable accuracy, provided the operator has a steady hand and good eye; but his work will ever be irregular and inelegant. He must have a circular line passing through the middle of his dots, to enable him to make and keep them at an equal distance from the centre. The bisectional arcs, also, which cut them across, deform them much; and, what is worse, the dots which require correction (about two thirds perhaps of the whole) will become larger than the rest, and unequally so in proportion to the number of attempts, which have been found necessary to adjust them. In the course of which operation, some of them grow insufferably too large, and it becomes necessary to reduce them to an equality with their neighbours. This is done with the burnisher, and causes a hollow in the surface, which has a very disagreeable appearance. Moreover, dots which have been burnished up are always ill defined, and of a bad figure. Sir George Shuckburgh Evelyn, in his paper on the Equatorial *, denominates these "doubtful or bad points;" and (considering the few places which he examines) they bear no inconsiderable proportion to the whole. In my opinion, it would be a great improvement of this method, to divide the whole by hand at once, and afterward to correct the whole; for a dot forced to its place, as above, will seldom allow the compass-point to rest in the centre of its apparent area; therefore other dots made from these will scarcely ever be found in their true places. This improvement also prevents the corrected dots from being injured, or moved,

Its defects.

* Phil. Trans for 1793.

by

by the future application of the compasses, no such application being necessary.

I will now dismiss this method of dividing, with observing, that it is tedious in the extreme; and did I not know the contrary beyond a doubt, I should have supposed it to have surpassed the utmost limit of human patience*. When I made my first essay at subdividing with the roller, I used this method, according to the improvement suggested above, of correcting a few primitive points; but even this was too slow for one who had too much to do. Perhaps, however, had my instruments been divided for me by an assistant, I might not have grudged to have paid him for the labour of going through the whole work by the method of adjustment; nor have felt the necessity of contriving a better way.

Its tediousness.

I might now extend the account of my method of dividing to a great length; by relating the alterations which the apparatus has undergone during a long course of years†, and the various manner of its application, before I brought it to its present state of improvement; but I think I may save myself this trouble, for truly I do not see its use: I will, therefore, proceed immediately to a disclosure of the method, as practised on a late occasion, in the dividing of

Mr. Edward Troughton's method

* At the time alluded to, the double microscopic micrometer was unknown to me, and I did not learn its use, for these purposes, till the year 1790, from General Roy's description of the large theodolite. Previous to that time, I had used a frame, which carried a single wire very near the surface to be divided; this wire was movable by a fine micrometer screw, and was viewed by a single lens inserted in the lower end of a tube, which, for the purpose of taking off the parallax, was 4 inches long. The greatest objection to this mode of constructing the apparatus is, that the wire, being necessarily exposed, is apt to gather up the dust; yet it is preferable to the one now in use, in cases where any doubt is entertained of the accuracy of the plane which is to receive the divisions.

† The full conception of the method had occupied my mind in the year 1778; but as my brother could not be readily persuaded to relinquish a branch of the business to me in which he himself excelled, it was not until September 1785 that I produced my first specimen, by dividing an astronomical quadrant of two feet radius.

a four-

a four feet meridian circle, now the property of Stephen Groombridge, Esq., of Blackheath.

described. The surface of the circle which is to receive the divisions, as well as its inner and outer edges, but especially the latter, should be turned in the most exact and careful manner; the reason for which will be better understood, when we come to describe the mode of applying the roller: and, as no projection can be admitted beyond the limb, if the telescope, as is generally the case, be longer than the diameter, those parts which extend farther must be so applied, that they may be removed during the operation of dividing. Fig. 1, Pl. I,

Apparatus. and 2, Pl. II, represent the principal parts of the apparatus; Fig. 1 showing the plan, and Fig. 2 the elevation; in both of which the same letters of reference are affixed to corresponding parts, and both are drawn to a scale of half dimensions. A A is a part of the circle, the surface of which is seen in the plan, and the edge is seen in the elevation. B B B is the main plate of the apparatus, resting with its four feet *a a a a* upon the surface of the arc; these feet, being screws, may be adjusted so as to take equal shares of the weight, and then are fastened by nuts below the plate, as shown in Fig. 2. C C and D D are two similar plates, each attached to the main plate, one above and the other below, by four pillars; and in them are centred the ends of the axis of the roller E. F and G are two friction wheels, the latter firmly fastened to B, but the former is fixed in an adjustable frame, by means of which adjustment these wheels and the roller E, may be made to press; the former on the interior, and the latter on the exterior edge of the circle, with an equal and convenient force*. At the extremities of the axis of the roller, and attached to the middle of the plates C and D, are two bridges, having a screw in each; by means of which an adjustment is procured for raising or lowering the roller respecting the edge

* Sufficient spring for keeping the roller in close and uniform contact with the edge of the circle is found in the apparatus, without any particular contrivance for this purpose; the bending of the pillars of the secondary frames and of the axis of the roller chiefly supplies this property.

of

of the circle, whereby the former, having its diameter at the upper edge about ·001 of an inch greater than at the lower edge (being, as before described, a little conical), it may easily be brought to the position where it will measure the proper portion of the circle.

The roller.

Much experience and thought upon the subject have taught me, that the roller should be equal to one sixteenth part of the circle to be divided, or that it should revolve once in 22° 30′; and that the roller itself should be divided into sixteen parts; no matter whether with absolute truth, for accuracy is not at all essential here. Each of such divisions of the roller will correspond with an angle upon the circle of 1° 24′ 22·5″, or $\frac{1}{256}$th part of the circle. This number of principal divisions was chosen, on account of its being capable of continual bisection; but they do not fall in with the ultimate divisions of the circle, which are intended to be equal to 5′ each.

Microscopes.

The next thing to be considered is, how to make the roller measure the circle. As two microscopes are here necessary, and those which I use are very simple, I will in this place give a description of them. Fig. 6 is a section of the full size, and sufficiently explains their construction, and the position of the glasses; but the micrometer part and manner of mounting it are better shown at II, in Fig. 1 and 2.

Micrometer.

The micrometer part consists of an oblong square frame, which is soldered into a slit, cut at right angles in the main tube; another similar piece nicely fitted into the former, and having a small motion at right angles to the axis of the microscope, has at one end a cylindrical guide pin, and at the other a micrometer screw; a spring of steel wire is also applied, as seen in the section, to prevent play, by keeping the head of the micrometer in close contact with the fixed frame. This head is divided into one hundred parts, which are numbered each way to 50; the use of which will be shown hereafter. A fine wire is stretched across the movable frame, for the purpose of bisecting fine dots. Two of these microscopes are necessary; also a third, which need not have the divided head, and must have in the movable frame two wires crossing each other at an angle of about 30°; this microscope is shown at I, Fig. 1. In the two first micrometers,

meters, a division of the head is of the value of about 0·2″, and the power and distinctness such, that when great care is taken, a much greater errour than to the amount of one of these divisions cannot well be committed in setting the wire across the image of a well made dot. The double eye glass has a motion by hand, for producing distinct vision of the wire; and distinct vision of the dots is procured by similar adjustment of the whole microscope.

Adjustment of the size of the roller.

The first step towards sizing the roller is, to compute its diameter according to the measure of the circle, and to reduce it agreeably thereto, taking care to leave it a small matter too large. The second step is, after having brought the roller into its place in the plate B B, to make a mark upon the surface of the circle near the edge, and a similar one upon the roller, exactly opposite each other; then carry the apparatus forward with a steady hand, until the roller has made sixteen revolutions. If, now, the mark upon the roller, by having overreached the one upon the circle, shows it to be much too large, take it out of the frame, and reduce it by turning accordingly. When, by repeating this, it is found to be very near, it may be turned about ·001 of an inch smaller on the lower edge, and so far its preparation is completed. The third and last step is, the use and adaptation of the two microscopes; one of these must take its position at H in Fig. 1, viewing a small well defined dot made for the purpose on the circle; the other, not represented in the figure, must also be fixed to the main plate of Fig. 1, as near to the former as possible, but viewing one of the divisions on the roller. With a due attention to each microscope, it will now be seen to the greatest exactness, when, by raising or depressing the roller, its commensurate diameter is found.

Use & adaptation of the two microscopes.

Apparatus for making the dots in the circle.

Fig. 3, Pl. II, is a representation of the apparatus for transferring the divisions of the roller to the circle. It consists of two slender bars, which, being seen edgewise in the figure, have only the appearance of narrow lines; but, when looked at from above, they resemble the form of the letter A. They are fastened to the main frame, as at W and Z, by short pillars, having also the off leg of the angle secured in the same manner; Y is a fine conical steel point for making

making the dots, and X is a feeler, whereby the point Y may be pressed down with a uniform force, which force may be adjusted, by bending the end of the bar just above the point, so as to make the dots of the proper size. The point Y yields most readily to a perpendicular action; but is amply secured against any eccentric or lateral deviation.

Primary dots.

The apparatus, so far described, is complete for laying our foundation, *i.e.* making 256 primary dots; no matter whether with perfect truth, or not, as was said respecting the divisions of the roller; precision in either is not to be expected, or wished; but it is of some importance, that they should be all of the same size, concentric, small, and round. They should occupy a position very near the extreme border of the circle, as well to give them the greatest radius possible, as that there should be room for the stationary microscope and other mechanism, which will be decribed hereafter.

It must be noticed, that there is a clamp and adjusting screw attached to the main plate of Fig. 1; but, as it differs in no respect from the usual contrivances for quick and slow motion, it has been judged unnecessary to incumber the drawing with it.

Method of making them.

Now, the roller having been adjusted, with one microscope H upon its proper dot on the circle, and the other microscope at the first division on the roller; place the apparatus of Fig. 3 so that the dotting point Y may stand directly over the place which is desigaed for the beginning of the divisions. In this position of things, let the feeler X be pressed down, until its lower end comes into contact with the circle; this will carry down the point, and make the first impression, or primary dot, upon the circle; unclamp the apparatus, and carry it forwards by hand, until another division of the roller comes near the wire of the microscope; then clamp it, and with the screw motion make the coincidence complete; where again press upon the feeler for the second dot: proceed in this manner until the whole round is completed.

Errours of these dots to be ascertained.

From these 256 erroneous divisions, by a certain course of examination, and by computation, to ascertain their absolute and

and individual errours, and to form these errours into convenient tables, is the next part of the process, and makes a very important branch of my method of dividing.

Method of doing this.

The apparatus must now be taken off, and the circle mounted in the same manner, as it will be in the observatory. The two microscopes, which have divided heads, must also be firmly fixed to the support of the instrument, on opposite sides, and their wires brought to bisect the first dot, and the one which should be 180° distant. Now, the microscopes remaining fixed, turn the circle half round, or until the first microscope coincides with the opposite dot; and, if the other microscope be exactly at the other dot, it is obvious that these dots are 180° apart, or in the true diameter of the circle; and if they disagree, it is obvious, that half the quantity by which they disagree, as measured by the divisions of the micrometer head, is the errour of the opposite division; for the quantity measured is that by which the greater portion of the circle exceeds the less. It is convenient to note these errours + or —, as the dots are found too forward or too backward, according to the numbering of the degrees; and for the purpose of distinguishing the + and — errours, the heads, as mentioned before, are numbered backwards and forwards to fifty. One of the microscopes remaining as before, remove the other to a position at right angles; and, considering for the present both the former dots to be true, examine the other by them; *i. e.* as before, try by the micrometer how many divisions of the head the greater half of the semicircle exceeds the less, and note half the quantity + or —, as before, and do the same for the other semicircle. One of the micrometers must now be set at an angle of 45° with the other, and the half-differences of the two parts of each of the four quadrants registered with their respective signs. When the circle is a vertical one, as in the present instance, it is much the best to proceed so far in the examination with it in that position, for fear of any general bending or spring of the figure; but, for the examination of smaller arcs than 45°, it will be perfectly safe, and more convenient, to have it horizontal; because the dividing apparatus will then carry the micrometers, several perforations being made in the plate B for the limb

limb to be seen through at proper intervals. The micrometers must now be placed at a distance of 22° 30′, and the half differences of the parts of all the arcs of 45° measured and noted as before; thus descending by bisections to 11° 15′, 5° 37′ 30″, and 2° 48′ 45″. Half this last quantity is too small to allow the micrometers to be brought near enough; but it will have the desired effect, if they are placed at that quantity and its half, *i. e.* 4° 13′ 7·5″; in which case the examination, instead of being made at the next, will take place at the next division but one, to that which is the subject of trial. During the whole of the time that the examination is made, all the dots, except the one under examination, are for the present supposed to be in their true places; and the only thing in this most important part of the business, from first to last, is to ascertain with the utmost care, in divisions of the micrometer head, how much one of the parts of the interval under examination exceeds the other, and carefully to tabulate the half of their difference.

The instrument should be engraved before it is divided.

I will suppose that every one, who attempts to divide a large astronomical instrument, will have it engraved first. Dividing is a most delicate operation, and every coarser one should precede it. Besides, its being numbered is particularly useful to distinguish one dot from another; thus, in the two annexed tables of errours, the side columns give significant names to every dot, in terms of its value to the nearest tenth of a degree, and the mistaking of one for another is rendered nearly impossible.

Tables of apparent errours.

The foregoing examination furnishes materials for the construction of the table of half differences, or apparent errours*. The first line of this table consists of two varieties; *i. e.* the micrometers were at 180° distance for obtaining the numbers which fill the columns of the first and third quadrant; and at 90°, for those of the second and fourth quadrant. The third variety makes one line, and was obtained with a distance of 45°: the fourth consists of two lines, with a distance of 22° 30′: the fifth of four lines, with

* If the table of real errours be computed as the work of examination proceeds, there will be no occasion for this table at all, but I think it best not to let one part interfere with another, and therefore I examine the whole before I begin to compute.

a dis-

a distance of 11° 15′: the sixth of eight lines, with a distance of 5° 37′ 30″: the seventh of sixteen lines, with a distance of 2° 48′ 45″: and the eighth and last variety, being the remainder of the table, consist of thirty two lines, and was obtained with a distance of 4° 13′ 7·5″.

Table of real errours.

The table of apparent errours, or half differences, just explained, furnishes data for computing the table of real errours. The rule is this; let a be the real errour of the preceding dot, and b that of the following one, and c the apparent errour, taken from the table of half differences, of the dot under investigation; then is $\frac{a+b}{2} + c =$ its real errour. But, as this simple expression may not be so generally understood by workmen as I wish, it may be necessary to say the same thing less concisely. If the real errours of the preceding and following dots are both +, or both —, take half their sum, and prefix thereto the common sign; but, if one of them is +, and the other —, take half their difference, prefixing the sign of the greater quantity: again, if the apparent errour of the dot under investigation has the same sign of the quantity found above, give to their sum the common sign, for the real errour; but if their signs are contrary, give to their difference the sign of the greater for the real errour. I add a few examples.

Example. 1.

For the first point of the second quadrant.

Real errour of the first point of the first quadrant	—	0·0
Real errour of the first point of the third quadrant	—	6·9
Half sum or half difference	—	3·4
Apparent errour of the dot under trial	+	12·2
Real errour	+	8·8

Example 2.

For the point 45° of the second quadrant.

Real errour of the first point of the quadrant	+	8·8
Real errour of the last point of the quadrant	—	6·9
Half difference	+	0·9
Apparent errour of the dot under trial	—	8·9
Real errour	—	8·0

Example

Example 3.

Point 88°·6, or last point, of the third quadrant	
Real errour of the point 84°·4 of the third quadrant	— 21·6
Real errour of the point 2°·8 of the fourth quadrant	— 2·9
Half sum	— 11·9
Apparent errour of the dot under trial	— 4·0
Real errour	— 15·9

Example 4.

Point 88°·6, or last, of the fourth quadrant.	
Real errour of the point 84°·4 of the fourth quadrant	— 21·6
Real errour of the point 2°·8 of the first quadrant	— 10·2
Half sum	— 15·0
Apparent errour of the dot under trial	+ 9·5
Real errour	— 6·4

It is convenient, in the formation of the table of real errours, that they should be inserted in the order of the numbering of the degrees on their respective quadrants; although their computation necessarily took place in the order in which the examination was carried on, or according to the arrangement in the table of apparent errours. The first dot of the first quadrant having been assumed to be in its true place, the first of the third quadrant will err by just half the difference found by the examination; therefore these errours are alike in both tables. The real errour of the first dot of the second quadrant comes out in the first example; that of the fourth was found in like manner, and completes the first line. It is convenient to put the errour of the division 90° of each quadrant at the bottom of each column, although it is the same as the point 0° on the following quadrant. The line 45° is next filled up; the second example shows this; but there is no occasion to dwell longer upon this explanation; for every one, who is at all fit for such pursuits,

pursuits, will think what has already been said fully sufficient for his purpose. However, I will just mention that there can be no danger, in the formation of this table, of taking from a wrong line the real errours which are to be the criterion for finding that of the one under trial; because they are in the next line to it; the others, which intervene in the full table, not being yet inserted. The last course of all is, however, an exception; for, as the examining microscopes could not be brought near enough to bisect the angle 2° 48′ 45″, recourse was had to that quantity and its half; on which account the examination is prosecuted by using errours at two lines distance, as is shown in the two last examples.

All the computations should be preserved.

When the table of real errours is constructed, the other table, although it is of no farther use, should not be thrown away; for, if any material mistake has been committed, it will be discovered as the operation of dividing is carried on; and, in this case, the table of apparent errours must be had recourse to: indeed, not a figure should be destroyed until the work is done*.

Respecting the angular value of the numbers in these tables, it may be worth mentioning, that it is not of the least importance; 100 of them being comprised in one revolution of the micrometer screw; and, in the instance before me, 5·6 of them made no more than a second. It is not pretended, that one of these parts was seen beyond a doubt, being scarcely $\frac{1}{50000}$ of an inch, much less the tenths, as exhibited in the tables; but, as they were visible upon the micrometer heads, it was judged best to take them into the account.

(To be concluded in our next.)

II.

On Platina and native Palladium from Brasil. By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S*†.

Platina not found in Europe till lately,

ALTHOUGH platina has now been known to mineralogists for more than sixty years, yet it had not been discover-

* This is a very useful hint, applicable on many occasions. C.

† Philos. Trans. for 1809, p. 189.

ed

ed in any other places than Choco and Santa Fé, whence it was originally brought, until about two years since Mr. Vauquelin discovered it in some gray silver ores from Guadalcanal in Estremadura. In analysing these ores, he found some fragments that contained as much as one tenth of their weight of platina, but he did not find it accompanied by any of the new metals, that have lately been discovered in the Peruvian ore of platina.

and then not accompanied with the new metals.

The specimen which I am now about to describe is derived from a third source, and it is rendered the more interesting by having grains of native palladium mixed with it. This new mineral has lately been received from the gold mines in Brasil, by H. E. Chev. de Souza Coutinho, ambassador from the court of Portugal, resident in this country; and I am in hopes that some account of it may be acceptable to the Royal Society, although the analysis must necessarily be very imperfect, from the small quantity to which my experiments have unavoidably been confined.

Some from Brasil mixed with native palladium.

The general aspect of this specimen is so different from the common ore of platina, that I could form no conjecture of what ingredients it might be found to consist. Its appearance was such indeed, as at first sight to induce a suspicion of its not being in a natural state, for it had very much the spongy form, which is given to platina from imperfect attempts to render it malleable by means of arsenic.

Appearance different from that of the common ore of platina.

One circumstance, however, occasions a presumption, that no art has been employed in giving the grains their present appearance; as upon close inspection many small particles of gold are discernible, but there is none of the magnetic iron sand, with which the Peruvian ore abounds, nor any of the small hyacinths, which I have formerly noticed as accompanying that mineral*.

But not owing to art.

It is very well known, that the common ore of platina in general consists of flattened grains, that appear so much worn at their surface, as to be in a considerable degree polished, and the roughness observable in some of the larger grains arises from concave indentations of a reddish brown or black colour. The Brasilian platina, on the contrary; has

Common ore of platina described.

Brasilian.

* Phil. Trans. for 1805, p. 318; or Journal, Vol. XIII, p. 119.

no polish, and does not appear worn; but most of the grains seem to be small fragments of a spongy substance; and even those which are yet entire and rounded on all sides present a sort of roughness totally different from that of the former, as their surface consists of small spherical protuberances closely coherent to each other, with the interstices extremely clean, and free from any degree of tarnish.

Analysed.

The first portion that I employed for solution was taken without any selection, and being digested with a small quantity of nitro-muriatic acid, two of the grains were acted on much more rapidly than is usual with platina, and seemed to give a redder colour than that metal alone. These grains were consequently taken out, washed, and reserved for separate examination, and the solution was allowed to proceed till the rest were entirely dissolved. By the addition of muriate of ammonia an abundant precipitate was formed of a bright yellow colour. This precipitate was evidently platina, and its colour satisfied me, that the grains had not been brought into their present state from Peruvian platina by means of arsenic; for where arsenic has been employed, I have observed that the iridium contained in that ore is rendered more soluble than before, and thence communicates its red colour to the precipitate.

No iridium.

From the grains thus examined, there appeared not to be any iridium dissolved, nor any black powder containing iridium undissolved.

Palladium.

I next endeavoured, by prussiate of mercury, to ascertain the presence of palladium; but though a precipitate which occurred indicated a certain quantity, it remained doubtful whether it was derived from the grains of platina themselves, or from the two small fragments that had been in part dissolved before they were separated from the rest.

No iron, or rhodium.

By addition of ammonia to the solution, no iron was precipitated; and when the solution was afterwards allowed slowly to evaporate, I could discern no crystals or colour that I could ascribe to the presence of rhodium. In short, it seemed that these grains are really native platina nearly pure.

A minute portion of gold.

In order to discover whether the grains themselves contained any portion of gold, I selected three of the largest

weighing

weighing together eight grains and a half; and after a solution and precipitation, as before, by muriate of ammonia, I added a solution of green sulphate of iron, and obtained a precipitate of gold. It was, however, far too small in quantity to be estimated with correctness, but certainly did not exceed the $\frac{1}{200}$ of a grain. This, it is to be observed, is another circumstance in which the present mineral differs from the Peruvian ore of platina, which I believe never contains (in the ore itself) the smallest quantity of gold.

Palladium.

In this experiment also, I tried to detect the existence of palladium in the solution, and by prussiate of mercury again ascertained its presence; but it was in too small quantity for estimating the proportion it bore to the whole mass.

The gold alloyed with silver.

It may deserve to be remarked, that though neither the Peruvian nor Brasilian grains of platina contain any silver, yet the gold which accompanies them is in each instance so much alloyed with silver, that from about thirty small scales of gold picked from Peruvian platina, weighing two grains, I obtained as much as four tenths of a grain of silver, or one fifth part of their weight.

Native Palladium.

Native palladium.

The two fragments, that had been separated from the first solution, next claimed my attention, and evidently deserved a careful examination. They were each placed in a drop of nitric acid, and each communicated a deep red colour, which, by the tests of prussiate of mercury and green sulphate of iron, I was satisfied arose from palladium. The smaller fragment was then divided, and one portion allowed to remain in the acid till it seemed completely dissolved, and the other examined by the blowpipe. The utmost heat that could be given appeared to have no effect; but when a small piece of sulphur was applied to it, it fused instantly; by continuance of the heat, it parted with the sulphur, and became completely malleable. In short, it perfectly resembled palladium; and it retained its brilliancy in cooling, I judged it to be nearly pure.

Nearly pure.

Afforded a trace of iridium.

But as the surfaces which had been acted upon by nitric acid had a degree of blackness, that might be owing to some insoluble impurity, I have since that time dissolved the

larger

larger fragment for the sake of discovering the cause of this appearance. Hot nitric acid dissolved by far the greatest part; but there remained a black powder on which a fresh addition of this acid alone had no farther effect. But when a drop or two of muriatic acid was added, the whole was very soon dissolved. By the addition of muriate of ammonia, it became evident from the precipitate, that the residuum was principally platina. But this precipitate, instead of being yellow, had the deep red colour, which is usually occasioned by the presence of iridium. The platina reduced from this precipitate was also too black for pure platina, and when it was again dissolved, the solution was of a deep red, and the precipitate by muriate of ammonia red, as before; so that although the grains of Brasilian platina appear to be free from iridium, as well as from many other impurities that form part of the Peruvian ore, yet the grains of native palladium that accompany them afford a trace of this ingredient, and occasion a presumption, that osmium and rhodium may hereafter appear, when we can obtain this mineral in larger quantity.

Perhaps too may contain osmium, and rhodium.

Since the whole weight of metal employed in the last experiment did not exceed $1\frac{2}{10}$ grain, it is in vain to attempt to estimate the proportion of the ingredients, but if I am near the truth, in considering the quantity of the red precipitate as about one fifth of a grain, of which less than half is platina, those who are best acquainted with the intense colouring power of iridium may endeavour to form a conception of the extremely small quantity that can be present.

Palladium distinguishable by fibres diverging from one extremity.

As soon as I had ascertained the existence of native palladium, I endeavoured, by examination of its external characters, to distinguish its appearance from that of the surrounding substances, and I found it by no means difficult, although no difference of colour could be discerned. Having remarked that the larger fragment appeared rather fibrous, and that the fibres were in some degree divergent from one extremity, I examined the remainder of the small specimen which had originally been given to me, and by this peculiarity of structure I soon detected a third fragment, which upon trial proved to be the same substance.

By

By favour of the Chev. de Souza I was also permitted, with this view, to examine the specimen which remained in his possession, and had soon the satisfaction of discovering two more fragments of the same mineral, and as I was in no one instance deceived in my choice, by attending to the radiating fibres, I am in hopes that this external character will enable persons to distinguish this metal, in situations where they have not an opportunity of deciding by chemical experiment.

III.

On the Identity of Columbium and Tantalum. By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S**.

Discovery of columbium and tantalum.

WITHIN a short time after the discovery of columbium by Mr. Hatchett, in 1801†, a metallic substance was also discovered in Sweden by Mr. Eckeberg‡, differing from every metal then known to him; and accordingly he described the properties by which it might be distinguished from those which it most nearly resembled. But although the Swedish metal has retained the name of tantalum given to it by Mr. Eckeberg, a reasonable degree of doubt has been entertained by chemists, whether these two authors had not in fact described the same substance; and it has been regretted, that the discoverers themselves, who would have been most able to remove the uncertainty, had not had opportunities of comparing their respective minerals, or the products of their analyses.

Their identity suspected.

Tantalum compared with oxide of columbium.

As I have lately obtained small specimens of the two Swedish minerals, tantalite and yttro-tantalite, from which I could obtain tantalum, and was very desirous of comparing its properties with those of columbium, Mr. Hatchett very obligingly

* Philosophical Transactions for 1809, p. 246.

† Phil. Trans. for 1802: or Journal, vol. II, p. 129, 176.

‡ *Vetenskaps Academiens Handlingar*, 1802, p. 68. *Journal des Mines*, Vol. XII, p. 245. or, *Journal*, Vol. III, p. 251.

obligingly furnished me with some oxide of the latter, which remained in his possession.

Considerable resemblance.

The resemblance was such in my first trials, as to induce me to endeavour to procure a farther supply of columbium; and by application to the Trustees of the British Museum, I was allowed to detach a few grains from the original specimen analysed by Mr. Hatchett.

The American and Swedish minerals much alike,

Notwithstanding the quantity employed in my analyses was thus limited, I have, nevertheless, by proportionate economy of the materials, been enabled to render my experiments sufficiently numerous, and have found so many points of agreement in the modes by which each of these bodies can or cannot be dissolved or precipitated, as to prove very satisfactorily, that these American and Swedish specimens in fact contain the same metal; and since the reagents I have employed are in the hands of every chemist, the properties which I shall enumerate are such as will be most useful in the practical examination of any other minerals, in which this metal may be found to occur.

in external appearance,

In appearance the columbite is so like tantalite, that it is extremely difficult to discern a difference, that can be relied upon. The external surface, as well as the colour and lustre of the fracture, are precisely the same; but columbite breaks rather more easily by a blow, and the fracture of it is less uniform, appearing in some parts irregularly shattered; nevertheless, when the two are rubbed against each other, the hardness appears to be the same, and the colour of the scratch has the same tint of very dark brown.

and component parts.

By analysis also, these bodies are found to consist of the same three ingredients; a white oxide, combined with iron and manganese.

Treated with alkalis.

Either of these minerals, when reduced to powder, is very readily acted upon by potash; but as the iron contained in them is not affected by alkalis, it appeared better to add a small proportion of borax.

Five grains of columbite, being mixed with twenty-five grains of carbonate of potash and ten grains of borax were fused together for a few minutes, and found to be perfectly incorporated. The colour was of a deep green, from the quantity of manganese present. The mass when cold could

be

be softened with water, and a portion of the oxide could be so dissolved; but it seemed preferable to employ dilute muriatic acid, which by dissolving all the other ingredients excepting columbium, left the oxide nearly white, by the removal of the iron and manganese that had been combined with it.

The muriatic solution having been poured off and neutralized with carbonate of ammonia, the iron was then separated by succinate of ammonia; after which the manganese was precipitated by prussiate of potash.

Products of columbite.

The products thus obtained from five grains of columbite, after each had been heated to redness, were nearly,

White oxide	4 grains
Oxide of iron	$\frac{3}{4}$
Oxide of manganese	$\frac{1}{4}$

but it cannot be supposed that *proportions* deduced from experiments made on so small a scale can be entirely depended upon, although the *properties* of bodies may be so discerned, nearly as well as when larger quantities are employed.

Products of tantalite.

An equal weight of tantalite taken from a specimen, of which the specific gravity was 7·8, yielded, by the same treatment,

White oxide	$4\frac{1}{4}$ grains
Oxide of iron	$\frac{1}{2}$
Oxide of manganese	$\frac{2}{10}$

The insolubility of the oxide in mineral acids.

The white oxides obtained from each of these minerals are remarkable for their insolubility in the three common mineral acids, as both Mr. Hatchett and Mr. Eckeberg have observed.

In muriatic acid they cannot be said to be absolutely insoluble; but they are not sufficiently soluble for the purposes of analysis.

In nitric acid they are also nearly, if not perfectly insoluble.

In sulphuric acid, when concentrated and boiling, the oxide of columbium may be dissolved in small quantity, and so also may the oxide obtained from tantalite.

The

Potash renders it soluble in water.

The proper solvent, as has been observed by Mr. Hatchett and Mr. Eckeberg, is potash; and as it is not required to be in its caustic state, I employed the crystallized carbonate of potash on account of its purity and uniformity. Of this salt about eight grains seemed requisite to be fused with one of the oxide obtained from either of these minerals to render it soluble in water.

Soda does not, except in large proportion, and with heat.

Soda also combines with the oxide, and may be said to dissolve it; but a far greater proportion of this alkali is necessary, and a larger quantity of water. And although a solution may have been effected that is transparent while hot, it very soon becomes opaque in cooling, and finally almost the whole of the oxide subsides combined with a portion of the soda in a state nearly insoluble.

Precipitated from the alkali by an acid.

When a solution of the white oxide, obtained from either of these minerals, has been made, as above, with potash, the whole may be precipitated by the addition of an acid, and will not be redissolved by an excess of sulphuric acid, of nitric, of muriatic, succinic, or acetic acids.

Soluble under certain circumstances by some vegetable acids.

But there is a farther agreement in the properties of these two minerals, which appears above all others, to establish their identity; for though they are both so nearly insoluble by any excess of the mineral acid , yet they are each completely dissolved by oxalic acid, by tartaric acid, or by citric acid; and the solution of each is subject to the same limitations; for if the precipitate has been dried, it is become intractable, and can scarcely be dissolved again till after a second fusion with potash.

Alkaline solution precipitated by galls.

If to the alkaline solution of either of them there be added infusion of galls, prussiate of potash, or hidrosulphuret of potash, no precipitate occurs; but when a sufficient quantity of acid has been added to neutralize the redundant alkali, the infusion of galls will then occasion an orange precipitate; but prussiate of potash causes no precipitate, nor does the hidrosulphuret precipitate the oxide, although the solution may become turbid from precipitation of sulphur by a redundant acid.

Precautions necessary in the application of this test.

The characteristic precipitant of columbium is consequently the infusion of galls; but in the employment of this test certain precautions are necessary. For as an excess

cess of potash may prevent the appearance of this precipitate, so also may a small excess of oxalic or tartaric acids prevent precipitation, or dissolve a precipitate already formed. A larger excess of citric acid seemed requisite for this purpose, and would also dissolve the gallate of columbium. In each case the precipitate may be made to appear by neutralizing the redundant acid; and for this purpose carbonate of ammonia should be employed: for although pure ammonia has no power of dissolving the oxide alone, yet the gallate seemed to be perfectly redissolved by that alkali.

When infusion of galls is poured upon the white oxide recently precipitated, and still moist, it combines readily, and forms the orange-coloured compound.

Prussiate of potash.

Prussiate of potash occasioned no change in an oxide that had been purified by a second fusion with potash; but it appeared to dissolve a small portion of the oxide, as infusion of galls, poured into the clear liquor, occasioned a cloudy precipitate of an orange colour, though no such precipate took place when the infusion was mixed with the same prussiate alone.

Hidrosulphuret of potash.

Hidrosulphuret of potash being added to the oxide, and heated upon it, impaired the whiteness of its appearance, and seemed to detect the remains of some impurity, which had not yet been removed by other means; but no appearance indicated the formation of a sulphuret of columbium.

The oxides perfectly similar, but the minerals differ in spec. grav.

From a careful repetition of these experiments upon each of the oxides, I see no reason to doubt of their perfect agreement in all their chemical properties; but there is nevertheless a very remarkable difference in the specific gravities of the two minerals from which they are extracted.

very considerably.

The specific gravity of columbite was ascertained by Mr. Hatchett to be 5·918; that of tantalite was found by Mr. Eckeberg to be 7·953; and I have every reason to suppose their results correct, since a small fragment of the former appeared upon trial to be 5·87, while a specimen of tantalite, weighed at the same time, was as much as 7·8. I should, however, observe, that the specific gravities of three other

other fragments borrowed for this purpose were not so high, that of one being 7·65, of another 7·53, and of a third so low as 7·15.

This cannot be owing to difference in their proportions.

It is evident, that no variation of mere proportion of the ingredients can account for an increase of specific gravity from 5·918 to 7·953, which are in the ratio of 3 to 4; for since columbite contains four fifths oxide, if the whole remaining one fifth part in weight of that oxide could be supposed added to the same bulk, without diminution of the quantities of iron and manganese, the specific gravity would not then exceed 7·1: and even if a weight equal to one third of the whole were thus added, without increase of bulk, still the aggregate would not quite equal the heaviest tantalite in specific gravity; but, on the contrary, the quantity of white oxide in this specimen certainly does not amount to six sevenths, and probably is not more than five sixths of the whole mass.

Perhaps from cavities, or mode of aggregation.

The only chemical difference, by which this circumstance could be explained, would be the state of oxidation, which my experiments cannot appreciate; but it may also arise in part from actual cavities in the mass of columbite, and in part from the state or mode of aggregation.

IV.

Inquiries concerning the Influence, that Light exerts on the Propagation of Sound. By MODESTE PAROLETTE *.

Light an important agent in natural phenomena.

OF all the objects that offer themselves to the contemplation of the chemist and natural philosopher light is the noblest. The action it exerts on all the combinations of matter, its extreme divisibility, the rapidity of its propagation, and the part it takes in what constitutes the life of organic beings, lead us to consider it as a substance acting the first part in the economy of nature.

* Journal de Phisique, vol. LXVIII, p. 346.

The

The magic power this emanation from the heavens exerts on our eyes, in exhibiting to us the spectacle of the universe, cannot be sufficiently admired: but its power is not confined to the organs of sight; all our senses are subjected to the action of light, and it is from this mode of conceiving its action, that I undertook the experiments, of which I intend to give an account to the academy. The part that I shall treat of at present relates to the effects resulting from the relations that subsist between the rays of light, and the vibrations of sonorous bodies†. The following observations first began to fix my thoughts on this subject.

It affects other senses besides that of vision.

Its connection with sound.

In 1803 I lived at Paris. Being accustomed to rise before day, to finish a work on which I had been long employed, I found myself frequently disturbed by the sound of carriages, as my windows looked into one of the most frequented streets in that city. This circumstance, which disturbed me in my studies every morning, led me to remark, that the appearance of daybreak peculiarly affected the propagation of the sound: from dull and deep, which it was before day, it seemed to me to acquire a more sonorous sharpness in the period that succeeded the dissipation of the darkness. The rolling of the wheels seemed to announce the friction of two substances grown more elastic; and my ear on attending to it perceived this difference diminish, in proportion as the sound of wheels was confounded with those excited by the tumult of objects quitting their nocturnal silence.

How first suggested to the author.

Struck with this observation, I attempted to discover, whether any particular causes had deceived my ears. I rose several times before day for this purpose alone, and was every time confirmed in my suspicion, that light must have a peculiar influence on the propagation of sound. This variation however in the manner in which the air gave sounds might be the effect of the agitation of the atmosphere, pro-

Confirmed in the opinion, that light assisted its propagation.

† I have traced out for myself a series of inquiries, that I conceive must lead to some important discovery. My object is to ascertain the action of light in the various phenomena, that take place in the elastic fluids around us. This object, which on the one hand is connected with the mechanism of our sensations, on the other embraces the results of those first combinations, that escape our apprehension.

Object of the author.

duced

duced by the rarefaction the presence of the sun occasioned; but the situation of my windows, and the usual direction of the morning breeze, militated against this argument.

It could not be owing to the wind.

The action of the wind might be supposed capable of increasing the propagation of sound, when, blowing in the line from the sonorous body to the ear, it could impart its own velocity to the vibrations of the sound; but it appeared to me, that its action must be null, when its motion was perpendicular to that line*. The velocity of sound too, which is 1065 feet per second, nearly excludes the influence of the wind, the action of which is slower, and operates only on large masses of the atmosphere†.

Sound has perhaps a peculiar medium.

Sound is propagated by infinitely small vibrations according to the theory of Mr. De la Grange‡; and it is probable, that this takes place in the particles of a very light elastic fluid of a peculiar nature, and which should not be confounded with the gasses, that compose what we know of the atmosphere.

* Hist. of the Royal Acad. of Sciences of Paris, 1738.

† Miscel. Phil. Mat. Societ. Turin, tom 1.

With respect to the action of wind on the propagation of sound, Mr. Perrault, formerly member of the Academy of Sciences of Paris, expresses himself as follows.

Wind cannot have much effect on sonorous vibrations.

"The invisible particles of bodies, which by their structure and configuration occasion their essential differences, are themselves composed of atoms still smaller, and less different in different substances than those particles Both the atoms and particles are endued with elasticity. When the particles are agitated in such a manner, that this elasticity comes to act, on their recoil they strike the particles of the air that touch them with the greatest velocity they can impress upon them, since it is produced by the spring of their elasticity; and this velocity is so great, it exceeds that which the air commonly has for withdrawing behind the substance that strikes it. Besides, as the space in which the spring has acted is extremely small, the air can pass this short space forward with greater facility than retire behind the atom. The particle of air struck advances a space equal to that, to which the spring has stretched, pushing that next to it, and so on to the ear. Hence the sound is propagated with so much velocity; and other agitations of the air, as the wind, prevent its propagation but very little, as they are too slow with respect to it." Hist de l'Acad., vol. 1, p 223.

‡ Inquiries concerning the Nature and Propagation of Sound, by Lewis de la Grange.

Full

Full as I was of the subject, I thought of means of confirming the reality of the phenomenon by the help of an instrument, which, placing me above all doubt both with regard to the variations of the atmosphere, and as to any illusion that might have deceived my senses, should give me a just measure of the increase of the propagation of sound by the influence of light.

An instrument to measure the propagation of sound.

The construction of this apparatus offered its difficulties. The principal was to subject to mechanical proof an object, of which we are accustomed to judge only by our senses. Whatever precision might be desired in such a research, I conceived I might derive some assistance from the experience acquired in practising music, to attain satisfactory results.

An experienced ear, the habit of playing on musical instruments, and the desire of doing right, appeared to me to add to the mechanical means of my apparatus that degree of accuracy, which is requisite in physical demonstrations. The following were my ideas and modes of proceeding.

In whatever manner the vibration of a body may be communicated to the elastic fluids surrounding it, it is certain, that the vibration of these fluids is always analogous to that of the particles of the sounding body. A chord stretched and struck with the finger vibrates in a given mode and time. This different regularity in its vibrations it is, that forms the duration of the sound, and the nature of the tone. When a chord vibrates, the tremulation of the circumambient air, which is analogous to the motion of the chord, can communicate itself to a similar chord, if the dimensions and tension are in corresponding proportions. It is a known fact, that if two strings, belonging to two instruments, be in unison, we cannot touch one, without the other's vibrating and emitting a perceptible sound. I thought I might avail myself of this property of elastic fluids, to determine the mode of my experiments.

The vibration of the elastic fluid analogous to those of the particles of the sounding body.

I took two violins of good quality, fitted them up with well chosen Naples strings, and had the pegs made with copper screws, that I might graduate the tones with precision. I placed these two violins horizontally on a plank ten

Apparatus described.

ten feet long, and eight inches wide. These two instruments having been tuned to the Paris diapason, on that string of one of the violins which is called the second, because it is the second in the neck, I placed a bit of paper, intended to serve as an index in the course of my experiments.

As it was necessary to be able to bring these violins nearer together, or place them farther asunder, and for their movement to be marked on the plank, that served as the base of the apparatus, I arranged them so that one should be fixed, while the other was movable. The fixed was that which had the paper on its second string, and a line corresponding to this string was traced on the plank. The other was movable by means of a very simple contrivance. A little wooden table was held on the plank by means of two grooves; the violin was placed on this so that it could not be shaken; and by means of a screw in the end of the plank I could slide this table with the violin on it backward or forward. An opening in the table parallel with the second string enabled me to mark on the plank the changes of place in my experiments, which I conducted as follows.

Its application. With the forefinger, the other fingers resting on the neck of the instruments, I pressed the second string till it touched the third, and then let it go instantaneously. This fingering, which was done at a place marked on the table, and was always uniform, produced an oscillatory motion, which was heard on the corresponding string of the other violin. The little bit of paper pointed out to me at a distance the vibration of this string, as I separated the two violins till the agitation of the paper became almost null, and at length ceased. This point was that of the limit of the vibration. I marked it on the table that served to support the apparatus, and numbered it 100. The space between the two strings, which were parallel to each other, I then divided into 100 equal parts; and these hundredths were divided into tenths each at the extremity of the table, that I might have thousandth parts in my scale. This first experiment, which was to furnish me a standard of comparison both for the scale of my apparatus, and for the differences

ences in the propagation of sound, was executed on the 14th of May, 1803. Taking the precaution to unite the meteorological observations with the indications of my apparatus, I noted in my pocketbook the degrees of the different instruments I employed at the moment of the experiment. The weather was calm and clear, and the sun shone into my room. The following are the results and meteorological observations of this first trial.

Trial of it.

May 1803.	Thermometer.	Barometer.	Hygrometer.	Apparatus.	Observations.
14	11° 51·8°F.	28·408 30·28 E.	39	100	This experiment, begun at 20′ after noon, was repeated several times. My apparatus always marked the same distance to a few thousandths.

Its scale.

The whole of the scale of my phonometer, which is the name I give my apparatus, answers to 2 met. 14 cent. [7 feet], and consequently each degree is equivalent to about 2 cent. [8 lines].

This done, to proceed methodically I conceived it right to lay down as a fixed principle, that the distance of 7 feet was the limit of the greatest propagation of sound in my apparatus under the influence of light.

Might not the effects of other causes be confounded with those of light?

I was now eager to repeat these experiments in the dark, in order to clear up my doubts of the difference there might be in the velocity and propagation of sound by night and by day. The apparatus I employed promised me results sufficiently conclusive from the length of its scale, and lightness of its motion. It appeared to me, that the least variation must be perceptible, and capable of strict proof, when I could depend on my own attention, and the niceness of my ear. One difficulty only presented itself to my mind, which arose from the influence, that variations in the temperature, weight, and humidity of the atmosphere might have on the phonometrical changes. I was aware, that the propagation of sound must be according to the nature and density of the elastic fluids it traverses; and I was apprehensive of being led into errour by a cause foreign to that

which

which was the object of my inquiry: but I also knew, from the experiments made in 1738 by Maraldi, De la Caille, and Cassini, and from those of Bianconi in 1740, that the thickest fog had been found to have scarcely any effect on the velocity of sound. To proceed more satisfactorily therefore, I determined first to examine by means of my apparatus and meteorological observations, the effect of changes in the atmosphere on the velocity and propagation of sound.

Trials of the effect of changes in the atmosphere.

These trials I made on the 9th, 12th, and 15th of September, the same year, each time about noon, and with striking differences in the state of the sky and atmosphere. The following table exhibits the results of these.

Sept. 1803	Therm Fahr	Barometer.	Hygrometer.	Phonometer.	Observations.
9	62·96°	30·37	58°	99·9	Half an hour after noon. Sky clear.
12	53·6	30·32	51·5	99·7	45′ after noon. Sky covered with thick clouds.
15	59·72	30·12	51	99·8	40′ after 1. Cloudy and threatening rain.

Trifling any.

The small differences in the results of these experiments, the greatest of which does not exceed three thousandth parts, appeared to me not ascribable to changes in the atmosphere. In my opinion they are the consequence of the imperfection of my method, which is incapable of mathematical precision. Not being able at the moment to procure more certain data for this discussion, I was inclined to think, that the common changes of the atmosphere could not effect such a change in the nature and arrangement of the particles of the elastic fluid, as to restrain or accelerate the agitation occasioned by sonorous bodies.

Experiments in the dark.

Nothing remained but to ascertain by strict trials the decrease of the propagation of sound in the dark, which should give the solution of my problem. I began my experiments the 20th of September following, and I chose a night and an

an hour best adapted by the darkness of the weather to show the effect of the absence of light.

The place of the experiment was lighted by a watch lamp of a particular construction, which afforded light enough to perceive the motion of the paper on the string, without the rays being able to diffuse themselves through the chamber. The following were the results.

Sept 1803	Thermometer.	Barometer.	Hygrometer.	Phonometer.	Observations.
20	53·24°	29·79	65	98·1	11 o'clock at night. The sky covered with clouds

I was satisfied with this trial. Eager to make it public, I spoke of it to some friends, who encouraged me to repeat these trials, and to attend particularly to this subject, which might prove highly interesting to the progress of science. Private affairs prevented me from prosecuting it at that time, and I did not resume it till about ten months after. The following are the results of the experiments then made, which tended only to confirm the preceding.

Table of comparative experiments.

July 1804	Thermometer.	Barometer.	Hygrometer.	Phonometer.	Observations.
3	66·2	28·89	73·5	100	About noon. The sky free from clouds
5	55	28·90	66·5	97·5	Quarter after 11 at night. The sky partly covered with clouds.
11	57	29·86	68	99·8	1 o'clock after noon. The weather fine.
14	62·24	29·87	51·5	99·4	Quarter after 1 P. M. Raining
18	74·66	28·90	62	98·4	Just after midnight. The sky cloudy.

General conclusion.

Such have been the constant results of my researches. The mean term of the degrees of propagation of sound without light in three different trials was found to be 0·98.

The

The difference between the propagation of sound that took place during the night from that in the day comes out two degrees of my scale, answering to about 16 lines. Whenever I made any experiments on this subject I always acted with the greatest caution, taking care to guard against the slightest inaccuracy. The subject has always appeared to me difficult and delicate; and without venturing to assert too much, I confine myself to an account of my thoughts and inquiries, happy to have removed a doubt, that concerns one of the most important branches of our knowledge.

Facts should be compared.

After having called the attention of natural philosophers to a discussion, which may serve to render the nature of luminous bodies better known, as well as to elucidate the nature of sounds, and to discover the mutual action of the imperceptible substances that surround us, it seems to me not improper, to compare together the facts, that appear to relate to this subject, and which may give rise to discussions calculated to explain the phenomena.

Night supposed more favourable to the propagation of sound, but merely from the absence of noise.

Hitherto night has been considered as more favourable than day to the propagation of sound. That this is the case with respect to our ears cannot be doubted: but this argues nothing against my opinion. We hear farther by night on account of the silence; and this silence contributes to it, while, according to the celebrated Euler, the noise of a wind favourable to the propagation of a sound may prevent the sound from being heard*. I have reason to think, that our ear has more aptitude for hearing sounds by day than by night, and this from the stimulant action, that light exerts on the nervous system: but this will not account for the

The ear hears best by day:

* History of the Academy of Sciences at Paris, 1738.

Distinction between sound and noise.

Sound and noise, which are the same thing with respect to the object of my inquiries, exhibit essential differences when considered with regard to our ears. By sound is to be understood that peculiar resonance, which proceeds from a sonorous body, and the tone of which we know. Noise I conceive to be an assemblage of several sounds.

When any sound predominates our ears can distinguish it from other sounds in harmonic proportion with it. In noise the harmonic sounds are confounded and lost. The celebrated Condillac, speaking of the nature of sounds in his treatise on Sensations, has distinguished the two by the definition of appreciable sounds and inappreciable sounds.

phenomenon

phenomenon exhibited by the paper, the motion of which depended on the tremulation of the air of the room. Is the atmospheric air more dense on the appearance of light than in darkness? Is this greater density of the air, or of the elastic fluid that is subservient to the propagation of sound, the effect of aeriform substances kept in this state through the medium of light? On this hypothesis we must suppose, that a greater density may take place without increasing the weight of the air; and we might ascribe the increase in the propagation of sound to a greater elasticity in the fluids diffused through the atmosphere. This would confirm the opinion of Dr. Priestley, who said, that sound was propagated in different gasses in the ratio of their density. But the rays of the solar light are inseparable from the calorific rays: their presence therefore, by raising the temperature, must produce a dilatation in the circumambient air, that seems to exclude the hypothesis of condensation. Besides, it is proved by the experiments of Mr. Perolle, inserted in the 3d vol. of the Memoirs of the Royal Academy of Sciences of Turin, that the propagation of sound is affected, not merely by the density of the different gasses it traverses, but by their nature.

but this will not explain the effect on the apparatus.

Priestley's opinion.

Mr. Perolle having placed an alarum watch in a jar which he filled successively with different gasses, retired gradually from the apparatus, stopping at the point where the sound was no longer audible to him. Proceeding in this manner he found, that the weight of a cubic foot of each of the gasses tried being,

Perolle found, that sound was not in the ratio of the density of the medium.

Carbonic acid gas	1030,
Oxigen gas	765,
Atmospheric air	720,
Nitrous gas	698,
Hidrogen gas	72,

the propagation of sound followed an order not always analogous to that of the density. It was as follows.

In

	feet.	feet Eng.*
In atmospheric air	59	98·88
carbonic acid gas ...	48·4	81·11
oxigen gas..........	66·5	111·45
nitrogen gas	the same	
hidrogen gas........	13	21·79

Oxigen best adapted to its transmission.

The results of this experiment seem to prove, that of the different gaseous substances oxigen is best adapted for transmitting the vibrations of sonorous bodies; and the equality of effect obtained with nitrous gas, which contains 0·56 of oxigen, gives reason to think, that the increase of the propagation of sound has a determinate measure, and that a given quantity of oxigen gas diffused in the atmosphere is sufficient, to carry it to its maximum†.

Is this the cause of the effect of daylight?

This reasoning appears to me the more satisfactory, as it may connect the results obtained by Mr. Perolle with those of my experiments. It is certain, that during the day, and under the influence of the light, the atmospheric air is more saturated with oxigen than during the night; but it remains to be proved whether this surplus of oxigen, which from the nicest eudiometrical researches cannot exceed a few hundredths, can be capable of producing such a remarkable change.

Or rather, is it not light that acts in oxigen and nitrous gas?

On the other hand, when it is proved, that the density of gasses is not the only reason of the acceleration of the progress of sound; and my experiments appear to demonstrate a certain influence in light; may we not consider the latter as the true cause of the increased propagation of sound in oxigen and nitrous gas; since we know, that oxigen has a great capacity for light, and that nitrous gas cannot be formed without the concurrence of this substance? Whatever may be the opinion of natural philosophers on this subject, it is certain, that the hypothesis reconciles my experiments

* I suppose the feet in the preceding column to be those of Turin. C.

Light has considerable effect in chemical action.

† The illustrious Dr. Bonvoisin, who, in his Elements of Chemistry, has taken every opportunity of pointing out to his pupils the influence, that light exerts on a number of chemical actions, particularly notices the necessity of the concurrence of light in all the combinations of nitrogen with oxigen. See vol. 1, art. Acide, nitrique.

with

with those of Mr. Perolle; and that together they may lead to very important researches.

The action of light not merely mechanical.

Light has a velocity 900000 times as rapid as that of sound. Whether it emanate from the Sun, and reach to our Earth, or act by means of vibrations agitating the particles of a fluid of a peculiar nature, the particles of this fluid must be extremely light, elastic, and active. Nor does it appear to me unreasonable, to ascribe to the mechanical action of these particles set in motion by the sun the effects its presence occasions in the vibrations that proceed from sonorous bodies. The more deeply we investigate the theory of light, the more we must perceive, that the powers by which the universe is moved reside in the imperceptible particles of bodies; and that the grand results of nature are but an assemblage of an order of actions, that take place in its infinitely small parts: consequently we cannot institute a series of experiments more interesting, than those that tend to develope the properties of light. On the least success of such inquiries we flatter ourselves with the presage of some important discovery: our organs of sense are so immediately connected with the fluid that enlightens us, that the notion of having acquired an idea of the mode of action of this fluid presents itself to our minds as the hope of a striking advance in the knowledge of what composes the organic mechanism of our life, and of that of beings which closely follow the rank assigned to the human species.

The greatest natural phenomena owing to the affections of the minutest particles.

V.

On the Chemical Action of simple Galvanico-electric Chains formed of Metallic Solutions, Water, or Acid, and a Metal; and on the Disoxidation of Metallic Oxides effected by these Means: by Mr. BUCHOLZ *.

Galvanic electricity.

THE electricity produced by the pile of Volta and galvanic chains has presented philosophers with the most surprising

* Annales de Chimie, June, 1808. p. 266. Translated from Gehlen's Journal, No. 17.

prising results, particularly with respect to chemistry. Who can forget the decomposition of water effected in a peculiar manner, the oxidations, the disoxidations, the hidrogenations first accurately observed by Ritter, as well as the decomposition of several acids, salts, &c.? We might have hoped, that chemists would have paid more attention to electricity, and that they would have endeavoured to derive more advantage from it; but this has not been done in the degree, which the importance of the subject appeared to demand. Ritter too has the merit of having pointed out to chemists the influence of the electric matter on chemical phenomena: and though the opinions he started in his work on the Electric System of Substances may not be altogether accurate as to the effect of the electric matter in chemical actions, yet we cannot wholly deny its influence; particularly as Sylvester showed in 1806, that the precipitation of one metal by another was nothing but a galvanico-electrical process*; which confirmed the opinion expressed by Ritter in 1800, though this opinion can be admitted only in the case of metallic vegetations, and not for every sort of reduction in the humid way; because a chain of two metals and a fluid is not always formed, when one metal is reduced by another, and yet the result does not remain a moment doubtful.

Its influence in chemical phenomena.

Precipitations of metals.

The influence of electricity in chemical processes, with respect to oxidation and disoxidation, is still more evident, when chains are formed with two fluids and one metal, as Ritter observed in 1800; and the reality of which he demonstrated on the occasion of a remark I made in 1804, that tin precipitated muriate of tin in a metallic form, when water was poured into a solution of muriate of tin, and a slip of this metal was immersed in the solution and the water at the same time. The utility and necessity of studying the influence of electricity could never be shown in a more determinate manner, than in the case in which Ritter has explained the most enigmatical phenomena in a natural and easy manner. I have lately been more sensible of the justice of this explanation, as I have confirmed it by

In galvanic chains of one metal and two fluids the metal displaces its own oxide.

Tin.

* See Journal, vol. XIV, p. 94.

examining,

examining, whether other metals were capable of forming similar chains with their solutions and water. I communicate these experiments, because I know of no one except Ritter, who has attended to these chains, and they have appeared to me interesting with respect to the particular circumstances accompanying them, though they are all founded on the same system.

Experiments with solutions of copper, another fluid, and copper.

Experiment with crystallized green muriate of copper.

1. Into a cylindrical glass I poured a solution of half an ounce of crystallized green muriate of copper in two ounces of water, and added with the greatest caution three ounces of distilled water, so that the liquors remained separate, one above the other. I then immersed in the two liquors a slip of polished copper, half an inch broad and six inches long, which rested on the bottom of the glass. At the expiration of two hours no action appeared to have taken place, except that the slip of copper was covered with a white crust, that continued increasing for twelve hours. On examining this substance, it was found to be white muriate of copper, formed by the oxide of the green muriate sharing its oxigen with the metallic copper immersed in the liquor.

Exp. with white muriate of copper.

2. As the preceding experiment had shown, that the green muriate of copper could not form a chain capable of reducing completely the oxide of copper held in solution, I was desirous of seeing whether the white muriate would comport itself differently. Accordingly I boiled a drachm of this salt with three ounces of water for a quarter of an hour, filtered it when cold, and placed the solution in contact with two ounces of distilled water and a slip of polished copper as in the preceding experiment. Having obtained no precipitate of copper at the expiration of a few hours, or even in some days, I ascribe this nullity of effect to the little difference between the specific gravity of the two fluids; for it is well known, that water dissolves very little white muriate of copper; whence the two fluids will unite immediately and destroy the chain.

3. Half

Exp. with nitrate of copper.

3. Half an ounce of copper was dissolved in nitric acid, taking care that there should be no greater excess of acid than was absolutely necessary for the solution of the salt. The solution being evaporated to two ounces, the chain was formed by adding three ounces of distilled water above it, and immersing in the two fluids the slip of polished copper, taking care to keep it a few lines from the bottom of the glass by fastening it to a bit of cork. The two fluids appeared at first quite separate from each other: but a few minutes after the chain was formed, a narrow but very bright transverse line appeared on the copper at the point of their separation; above this point the copper was much darker; and at the end immersed in the fluid, small and almost imperceptible filaments were deposited, which gradually increased, but ultimately disappeared. The bright line increased greatly, and the part that was in the water grew darker, till it became a brown black. At the end of 72 hours, when the two liquids appeared to be completely mixed, and the activity of the chain destroyed, I took out the slip of copper, and found it in the following state. Near the centre there was a bright place about a quarter of an inch broad, where neither oxidation nor precipitation of the copper was perceivable. Above this line was a slight coat of brown black oxide of copper; and below a red pulverulent stratum of copper, which gradually thickened toward the end of the slip, and assumed a striated appearance. This stratum, viewed with a lens, appeared to be formed of a collection of little grains, which acquired a metallic lustre on being rubbed. The result of this experiment showed evidently, that, under suitable circumstances, a very active electro-galvanic chain might be formed in this manner, by means of which copper might be precipitated in the metallic state by means of copper itself.

Nitrate of copper and water acidulated with nitric acid.

4. To learn what would be the reductive power of such a chain, if the water employed were acidulated with $\frac{1}{16}$ of nitric acid, I proceeded nearly as in the foregoing experiment. The result was much the same, only the slip of copper was more speedily oxided in the acid liquor, some bubbles of gas escaped, and the copper was deposited in a thicker stratum, and with a lustre almost metallic, at the bottom

bottom of the slip. The experiment terminated in much less time.

5. To vary the third experiment, I made a solution of copper in nitric acid, so as to leave some metallic copper in the saturated solution; and then formed a chain. At the expiration of a few minutes the usual phenomena presented themselves, except that at the point of contact of the two liquors a slight turbidness appeared, occasioned by the precipitation of the nitrate of copper at a minimum in the form of small flocks of a greenish white. At the commencement some flocks were deposited on the edges of the slip of copper, but they afterward disappeared. As, when water had been added, the liquor was rendered absolutely turbid by the separation of the oxide of copper, which was present in excess, I took out the slip of copper, and was agreeably surprised by an unexpected result: as far as the slip had been immersed in the cupreous solution, it was covered with another slip of copper extremely thin, on which were several little knobs of the size and shape of pin's heads, which looked metallic, smooth, and as if they had been melted, and viewed with a lens had one or two little openings on the side. It appeared to me too, that the flocks observed on the edges of the slip had been converted into similar little knobs, for the edges were covered with them in different places. Whence could arise this interesting phenomenon of the separation of the copper in the form of little globules?

Nitrate of copper with a little excess of metal.

6. This experiment was again varied by allowing the solution of copper to contain a slight excess of acid. The result was the same as in experiment 3, except that the copper was longer in being precipitated; this not taking place till the excess of acid appeared to be deadened, which was effected with the extrication of a great many bubbles of gas; and the separation took place only at the bottom of the slip, and in the form of little grains.

Nitrate of copper with excess of acid.

Many other experiments, infinitely varied in order to obtain more extensive cupreous vegetations, taught me, that the reduction of copper by means of copper was more complete in proportion as the solution was more concentrated, the point of saturation more exact, and the slip of copper broad,

Reduction of copper most complete with a saturated & perfectly neutral solution,

and accelerated by slightly acidulating the water.

broad, thick, and polished; and that when a little nitric acid was added to the water the result of the operation was accelerated by the oxidation of the metal being effected more copiously in the superior fluid: but that a too great excess of acid was unfavourable to its success, the process going on with too much commotion, and producing too speedy a mixture of the liquors, as well as too great a specific gravity of the upper, which destroys the action of the chain.

Experiments with the solution of nitrate of silver, water, and silver.

Exp. with nitrate of silver.

7. Into a small narrow glass I put a solution of two drachms of nitrate of silver in half an ounce of water, and poured on it six drachms of distilled water, acidulated with half a drachm of pure nitric acid. The chain was completed by a little piece of solid silver, narrow and ending in a point, which was fixed in a bit of cork. The action was almost instantaneous. A point of 0 was formed, and marked by a very bright transverse line, that remained intact. Immediately below this point metallic silver separated in shining grains resting one against another in a horizontal line, which continued to be deposited the whole length of the slip, which was half an inch. These grains increased for twelve hours below the point of 0. The slip of silver became gray, and was gradually covered with black oxide. At the expiration of this time the action of the silver was no longer perceptible, and the slightest touch occasioned it to drop asunder, as it was corroded where the acid liquor was in contact with the air. During the whole process there were but few bubbles extricated in the upper liquor. The slip of silver covered with little shining knobs exhibited a pleasing sight, when viewed either with a lens or the naked eye. Professor Bernhardi examined them with a microscope, which magnified 200000 times, and then distinctly perceived, that they were cubes truncated at the edges.

The same with a larger quantity.

8. I repeated the preceding experiment with a view to effect a more considerable separation of silver. Accordingly I dis-

I dissolved an ounce of fine silver in pure nitric acid, and added water so as to form a saturated solution weighing six ounces. This solution I poured into a cylindrical vessel, and very cautiously added eight ounces of water. I then completed the chain with a pointed slip of silver six inches long, and one inch broad. The action was not perceptible till after three or four hours, when a point of O was observable, above which the metal became gray, while below single grains of silver were deposited, so small as to be scarcely distinguishable by the eye. In 72 hours these points had acquired the size of pin's heads in seven or eight places. The upper part of the slip was turned of a deeper gray, but the two liquors being perfectly mixed, there were no hopes of obtaining a more complete result.

Activity in proportion to the oxidation.

This experiment affords fresh proof, that the activity of a chain bears a certain ratio to that of the oxidation.

Nitrate of silver and water acidulated with nitric acid.

9. The liquors of the preceding experiment were evaporated to six ounces, and after being returned into the same vessel, five ounces of water acidulated with one of pure nitric acid of the specific gravity of 1·25 were added with a great deal of caution. The chain was completed with the slip of silver that has been mentioned. The action began immediately. A point of O was apparent a quarter of an inch broad, where the slip of silver remained unaffected. Above this point was formed black oxide; and beneath, throughout the whole breadth of the slip, a line of knobs of shining silver, exactly as in experiment 7, but in a somewhat larger proportion. These little grains increased for 36 hours, as well as the black oxide. The silver separated had the same form as in experiment 7, except that some agglutinations of silver at the point of the slip were dull, though they became shining on being pressed upon with a hard substance. On taking out the slip at the expiration of 36 hours, it divided into two parts. It was particularly corroded where it had been in contact with the liquid at the upper part, at which place it was completely converted into black oxide.

Experiments

Experiments with solutions of lead, water, acidulated water, and lead.

Exp. with nitrate of lead.

10. A chain was formed with four ounces of a solution of nitrate of lead made without heat, four ounces of water, and a slip of lead an inch broad, six inches long, and half an inch thick, fixed in a piece of cork laid across the top of the glass. Immediately a point of 0 was perceived, where the liquors were in contact, and this continued growing broader and more perceptible during the course of an hour. Above this point the brightness of the lead continued diminishing; and below small shining metallic laminæ were deposited, which increased for 24 hours. At the end of this time the liquors had mixed, and destroyed the chain. The metallic laminæ, which were pure lead, had no regular figure.

Nitrate of lead, & water acidulated with nitric acid.

11. To learn what would be the action of a similar chain, if acidulous water were employed, I repeated the experiment with the addition of two drachms of acid to the water. Soon after a brighter place was observed where the two liquors were in contact; above this gas was extricated, and white oxide was formed; while below the slip of lead grew dull. After the expiration of twenty-four hours oxide was formed at this place also, and gas was evolved. At this period the liquors had mingled together, without any separation of metallic lead. According to all appearance there had been no very active chain, because the liquors mixed too soon. To verify this supposition, I made the following experiment.

Warm solution of nitrate of lead and acidulated water.

12. Four ounces of boiling distilled water were saturated with nitrate of lead, and put milk-warm into a cylindrical glass. Four ounces of water acidulated with an ounce of nitric acid at 1·25 being added upon the solution, the chain was completed with a slip of polished lead. A point was formed, that appeared more shining than in the preceding or following experiments. The part of the lead in the acidulous water was covered with a great many bubbles of gas, and became gray. On the edges of the slip that were immersed in the solution were deposited filaments, which at the expiration of some hours increased, so as to form

little

little eminences of scales of lead a third of a line long; and at the same time crystals were deposited, which were in part attached to the slip of lead, and in about twelve hours had covered the slip, and formed a fine groupe interspersed with scales of lead. It was only near the point of 0, where the solution was weakened by the supernatant liquor, that no crystals were deposited with the scales of lead. The upper part of the slip of lead was covered with gray oxide.

The specific gravity of the fluids should differ.

The result of this experiment showed I had not conjectured without reason, that no lead was separated in experiment 11 because the specific gravity of the liquors differed too little, which occasioned the electric fluid to be badly conducted, and the liquors to mix too soon and destroy the chain.

Exp. with acetate of lead.

13. A solution of an ounce of acetate of lead in three ounces of distilled water, forming a chain with five ounces of water and a slip of lead, had experienced scarcely any change at the end of 24 hours, except that the lead appeared a little duller toward the bottom, and a little brighter toward the top.

Acetate of lead and distilled vinegar.

14. I evaporated the superfluous water from the solution of lead, and employed as the upper liquor distilled vinegar. A few minutes after the chain was completed the point of 0 was perceptible, above which the slip of lead became of a whitish gray, and below it several points of a blackish gray were formed. The latter, after the expiration of 36 hours, had increased so as to cover almost the whole of the lead immersed in the liquor. These points, examined with more care, exhibited the form of little knobs. They were friable, which led me at first to take them for oxidule of lead; but having rubbed them with a hard substance they acquired the metallic brilliancy, which showed, that they were metal.

Experiments with muriate of zinc, water, acidulated water, and zinc.

Muriate of zinc.

15. I formed a chain with half an ounce of zinc dissolved in muriatic acid and diluted so as to form four ounces of solution, five ounces of water, and a slip of zinc. Soon after

after the chain was completed, a blackish gray substance was deposited on the point of the zinc immersed in the solution, and this continued increasing for 48 hours, while a grayish coat of oxide of zinc was depositing on the part in the water. The ramifications of zinc, that were on the lower part, had the aspect of an oxide; but on rubbing them with a hard substance they acquired the brilliancy of zinc.

Muriate of zinc and acidulated water.

16. This experiment was repeated, only acidulating the water with two drachms of muriatic acid. The result was nearly the same, except that less zinc separated, because the chain did not remain so long active, the liquors having mingled too soon.

Experiments with solutions of iron, water, acidulated water, and iron.

Green sulphate of iron.

17 and 18. An ounce of green sulphate of iron was dissolved in three ounces of water, and a chain was formed with four ounces of water, and a bar of very soft iron. I observed no difference between the action that took place on the part of the bar in contact with the water, and that in contact with the solution of iron, but in both the iron was coated with a stratum of black oxide of iron, and some yellow flocks resembling oxide separated from the solution. The same effect took place when the water was acidulated with a drachm of concentrated sulphuric acid. In neither of these experiments was any electric polarity observed, or point of 0.

Muriate of tin.

19. Four ounces of fuming muriatic acid were gradually saturated with pure iron filings. With this solution, four ounces of water, and a bar of soft iron, a chain was formed. It did not appear to have more effect than those of the two preceding experiments, for no difference was observable in the state of the iron in the upper and under liquors. In each the bar of iron was covered with black oxide, and flocks of brown yellow oxide were gradually deposited.

Muriate of iron and acidulated water.

20. On varying this experiment by adding two drachms of fuming muriatic acid to the four ounces of water, a solution of the iron in the acidulated water took place, with the evolution

evolution of gas. The iron in contact with the solution was covered with a coat, that had sometimes the colour of indigo or of copper, and acquired the brightness of iron by rubbing; which appeared to indicate a separation of iron in the metallic state, and in a pulverulent form, though I am not inclined to adopt this opinion. The too speedy mixture of the two liquors having destroyed the chain, I could not procure myself enough of this black powder to make an accurate examination of it, and to determine what it was with more certainty.

Conclusion.

General deductions.

From all the experiments I have described it appears to me we may infer, that almost all metals are capable of forming a chain with their own solutions and water, the electric action of which precipitates the metal in the metallic state; and that an evident anomaly appears to take place, as if the metal dissolved were precipitated by the pure metal, which would seem contradictory to the received laws of affinity between a metal and oxigen. It has always appeared to me, that the following conditions are necessary to our complete success.

The acid must be one that acts but little on the same metal.

1. The metal must form a solution with some acid, which is altered very slowly or not at all by the pure metal; and which consequently contains no excess of acid or of oxide; in order that the electric current formed by the chain may act without being checked on the metallic oxide in the solution.

The solution must be strong:

2. The solution must be sufficiently concentrated, not to mix easily and readily with the supernatant liquor, and annihilate the chain.

and the metal must be oxidable by water.

3. The metal, that is to form the chain, should be oxidable by water, and thus determine the electric current so as not to occasion a mixture of the two liquors, and thus destroy the chain too quickly.

Other metals.

It is to be presumed, that gold, platina, bismuth, manganese, tungsten, and other metals, would likewise form electro-galvanic chains, and exhibit the apparent anomaly of a piece of metal precipitating the oxide of the same me-

tal from its solution, which I will examine when I have leisure, unless I am prevented by others.

Addition to the preceding paper.

The results confirmed.

I have farther to remark, that the results of my experiments on the precipitation of copper confirm an observation announced by Mr. Bucnger; but for more certainty I made the following experiment.

Farther exp. with sulphate of copper.

21. I formed a chain by putting into a cylindrical vessel a solution of two ounces of sulphate of copper in six ounces of boiling water, on this I poured six ounces of water, and I immersed in the two liquids a slip of copper. At the expiration of 24 hours no change appeared on the slip of copper, either in the cupreous solution, or in the water. Some sulphate of copper however was separated, and the two liquors were well mixed.

Sulphate of copper and acidulated water.

22. I repeated the former experiment exactly, except that I added an eighth of sulphuric acid to the water employed. The result was, as soon as the chain was formed an oxidation and disoxidation of the metal took place, and a point of 0, $\frac{1}{3}$ of a line broad, formed on the slip of copper. At the expiration of 48 hours, as the action no longer increased at the two poles, I took out the slip of copper, and found, that the whole of the part immersed in the solution was covered with a solid but thin coat of copper, which viewed in front had the colour of pure copper, but dull and without brightness; but viewed obliquely had a velvety appearance, and a paler colour. Seen through a lens this crust appeared crystalline, and of a metallic brightness. The slightest rubbing gave it the polish of the purest copper. Between the part covered with this coppery incrustation and the oxided part there was a stripe a third of a line broad, where the slip of copper remained unaffected; and above this, for the space of two inches, the copper was covered with black brown oxide.

VI.

VI.

Extract of a Letter on Potassated Iron from Mr. HASSENFRATZ, *Engineer in Chief of Mines, and Professor of Mineralogy at the Practical School of Mines, to Mr.* GILLET-LAUMONT, *Correspondent of the Institute, and Member of the Council of Mines**.

YOU are acquainted with the beautiful experiments, in which Gay-Lussac and Thenard have decomposed potash and soda by means of iron, and obtained metals that combine readily with iron, producing alloys, from which nitrate of potash is obtained when they are treated with nitric acid. You know too, that crude iron is pretty generally obtained in France by fusion with wood charcoal, which contains more or less potash; and that this is afterward refined with the same combustible to produce malleable iron; whence it is highly probable, that the potash contained in the charcoal is reduced, and afterward combines with the iron in the processes it undergoes.

Alkalis decomposed by iron.

Iron smelted with charcoal.

Oak charcoal yields about eight parts of saline matter in a thousand, beech five, elm twenty, poplar six, fir two, &c. A mean of the charcoal commonly employed will produce seven parts of salt to a thousand of fuel; and if 500 parts of charcoal be used on a medium to 100 of iron, it follows, that two or three per cent of the new metal may combine with the iron formed.

Proportions of potash in charcoal.

The small quantity of the new metal that can combine with iron in the different processes it undergoes might remove the apprehensions of those metallurgists, who are inclined to ascribe injurious effects to it, that quantities as inconsiderable of phosphorus, sulphur, copper, &c., will render iron, the first coldshort, the other two redshort.

Iron injured by small quantities of alloy.

From the small combinations of potassium and iron hitherto made, it is difficult to form a judgment of the influence of this new metal on the goodness of iron, because the quantities obtained were too small to be forged: but notwithstanding the smallness of the buttons of potassated iron that have been fused, some men of science have thought, that the compound was brittle. When coldshort iron is purified at Zinswiller on the Lower Rhine with lime and potash, iron of good quality is obtained, if the quantity of the

Trials should be made on a large scale,

and these seem to show, that too much potash injures iron.

* Journal des Mines, vol. XXIII, p. 275.

the substances added in the refining be suitable: but the iron becomes redshort; if they be employed in too large proportion, whence it follows, that lime and potash are capable of rendering iron brittle.

Does it render it redshort?

The opinion of some scientific men, who presume that the action of potash must render iron brittle; and the experiment in the large way at Zinswiller, which would induce a belief, that potash occasions it to be redshort; would lead metallurgists to ascertain by direct and positive experiments the influence of potash in its combination with iron.

The author writing on the art of iron founding.

Being at this moment engaged by order of his excellency the minister of the home department in writing on the art of treating and preparing iron, it was natural for me to attend to this question. Accordingly I requested the two respectable chemists, to whom we are indebted for the discovery of the reduction of potash by means of iron, to favour me with one of the gunbarrels, in which they had repeated their experiments a great number of times at the Polytechnic School. They had the goodness to choose me one, the inside of which was interiorly filled with potassated iron, and which the combination of potash had rendered so fusible, that it had in part melted during their experiments.

A gunbarrel that had been repeatedly used for reducing potash,

examined.

The iron of this gunbarrel, penetrated with potash, was assayed in presence of one of the pupils and myself by the locksmith Rosa, who is commonly employed by the Council of Mines for the various trials of iron and steel.

Heated and flattened.

The gunbarrel was heated and hammered flat for the length of four inches on each side of the part fused by the combined action of the potash and heat.

Piece of alloy broken off.

In flattening it a portion of superpotassated iron was separated, which we picked up. This little piece, weighing 4 gr. [62 grs.], was coldshort. In its fracture it exhibited a mixture of a brown substance and a white. The latter had the metallic lustre, and was pretty easily hammered when separated from the brown substance. After 24 hours exposure to the air the small piece was covered with moisture, which it had attracted from the atmospheric air.

Its properties.

The barrel forged and welded, bent and twisted hot

The barrel after being flattened and heated was forged and welded very perfectly. It extended under the hammer as easily as coldshort iron. It was bent and twisted backward and forward several times, without the least crack appearing.

A small

A small bar four lines thick was bent backward and forward cold, but it broke after repeated bending. The outer surface of the iron was a dull platina white. The fracture exhibited a white rim, fine grained, and strongly compressed, from 8 to 15 tenths of a line broad; and this rim enclosed a brown nucleus from 2 lines to 2·75 broad. The nucleus, completely separate from the rim, and having but a slight adhesion to it, was composed of white and brown grains weakly united. It was formed of the interior surface of the barrel, which had been most exposed to the action of the potash. The outer iron was soft, and easily filed and flatted cold.

and cold.

Broke.

Another bar forged thin was tempered, after being heated to a cherry red. Its surface was hard when filed, and the metal retained its malleability. It did not break, till it had been bent several times. Its fracture appeared similar to that of the iron which had not been tempered.

Tempered.

From all these trials we may conclude, that the iron appears to have combined with the potash in the gunbarrel in two different proportions; one a minimum, which gives iron a white colour resembling that of platina; the other a maximum, which gives it a brown colour mingled with white specks:

The iron combined with potassium in two proportions:

That the iron potassated at a maximum is easily wrought hot or cold, that it becomes more malleable by this combination, and that it can acquire hardness by tempering without becoming brittle like steel; consequently it is highly probable, that the small quantity of potassium, which combines with iron when it is smelted with charcoal, can only contribute to improve its quality:

a small proportion improved,

That iron potassated at a maximum acquires a brown hue more or less deep, mingled with larger or smaller white specks; and that in this state the particles have less cohesion, so that the iron becomes coldshort, and very probably redshort:

a large injured it:

A single experiment on the combination of potash with iron may give some idea of the new properties the metal acquires during and after it is wrought; but it would be too hasty to conclude absolutely respecting the qualities of potassated iron from a single trial. We must wait therefore till further experiments shall confirm the whole of the results we obtained, refute some of them, or add new ones, when we may decide with more certainty.

but one trial not sufficient.

VII.

VII.

Analysis of the Aerolite that fell at Stannern in Moravia, the 22d of May, 1808. By Mr. VAUQUELIN*.

Meteoric stone of Stannern.

THIS aerolite in its external appearance resembles other productions of the kind. It is covered externally with a brown, vitreous coating or glaze. Interiorly it exhibits a gray substance, interspersed with black dots; and in several places there are shining laminæ, that appear to be pyrites, for they are not attracted by the magnet; nor does the stone itself act on the magnetic needle. This substance is not homogeneous, for pretty considerable nodules, much blacker than the rest of the stone are visible in it to the naked eye.

Contained the same principles as basaltes.

Mr. Klaproth, having analysed a specimen of this mineral which was sent him in powder, and found it to contain the same constituent principles as basaltes, and nearly in the same proportions, was desirous of a description of it in substance, in order to compare it with basaltes. Accordingly Count Unin, who has a very fine specimen of it, has given the following description.

Its appearance.

The surface of the meteorolite of Stannern is fused; and perfectly black. This characteristic, which is peculiar to meteorolites, distinguishes them from other stones. The colour of its substance is a light ashen gray, which is not altered by scraping. Interiorly are perceived more compact grains, and of a darker colour than the rest of the mass. It contains likewise grains of sulphuret of iron, but in small quantity.

Its properties.

This stone is soft, friable between the fingers, not scratching glass, and giving no sparks with steel. Its specific gravity is 3·19. It fuses with difficulty before the blowpipe into an opake glass, which is attracted by the magnet.

Contains a

From the specimen I possess, which I picked up on the

* Annales de Chimie, vol. LXX. p. 321.

spot,

spot, the meteorolite of Stannern differs from others only in containing a smaller quantity of metallic substances.

proportion of metal,

Mr. Klaproth, from its chemical analysis, suspects a similitude between this stone and basaltes, but it is very certain, that the two substances differ effentially in their fracture, hardness, and appearance when scraped.'

but does not resemble basaltes.

The meteorolite that fell near Eggenfield in Bavaria, in December, 1803, is that which in its external characters comes nearest to the basaltic tufa in the neighbourhood of Klosterlaach. Mr. Chladni possesses a specimen of it, which is very remarkable from containing olivine, or the granular peridot of Haüy, disseminated in it.

Stone of Eggenfield more like basaltes.

Contains olivine.

Mr. Moser, a chemist of Vienna, found in 100 parts of this aerolite the following substances.

Component parts of the stone of Stannern according to Moser.

Silex	46·25
Alumine	7·62
Black oxide of iron	27
Oxide of manganese	0·75
Lime	12·12
Magnesia	2·50
A trace of chrome, and of muriates	
Sulphur, water, and loss	3·76
	100

These results, so different from what have hitherto been found in productions of this kind, having led some persons to suspect, that there may have been errours in this analysis; and conceiving at any rate, that it deserved a repetition; requested me to undertake it, which accordingly I did.

Analysis of it.

Treated by muriatic acid, this stone gave out a very small quantity of sulphuretted hidrogen.

Treated with muriatic acid.

If it be subjected to a strong red heat, its colour changes to a pale red, but its weight is not diminished; no doubt because the oxidation of the iron makes up the loss of weight.

Heated.

Treated with caustic potash we obtain a fused substance, which has a green hue. This colour is more distinct on diluting the substance with water, which produces a deep green solution. After the solution is filtered, on being exposed

Treated with caustic potash.

posed to the air a slight flocculent precipitate of oxide of manganese falls down. On filtering afresh the solution has a fine yellow colour, which would be supposed to be owing to chrome. It was then saturated with nitric acid, and evaporated to dryness. During this operation it assumed the appearance of a jelly, which indicated the presence of silex. The nitrate dried, and redissolved in water, communicated to it but a very slight tinge. The silex, which had been separated, was perfectly white. The solution of nitrate of potash, tried in all ways with solutions of silver, mercury, and lead, even after having been considerably concentrated, did not exhibit the least sign of the presence of chrome. It appeared therefore, that the yellow colour of the solution was owing to a small quantity of platina taken up by the potash from the crucible in which the stone had been treated.

Silex.

No chrome.

Yellow colour from the platina of the crucible acted on by the potash.

Residuum.

The residuum, which had been separated from the green alkaline solution, was diluted in water, and supersaturated with muriatic acid, which dissolved it completely. This solution, which had a fine yellow colour, was evaporated to dryness, then diluted in acidulous water, and filtered. The silex, collected on a filter, was perfectly white, and was added to that before obtained.

Silex.

Muriatic solution.

The muriatic solution freed from silex was of a lemon colour. Being decomposed by ammonia in considerable excess, a very bulky brown precipitate was formed, which was collected on a filter. To the ammoniacal liquor oxalic acid was added, which produced a very copious precipitate of oxalate of lime. This was carefully filtered. Though the solution was colourless it was evaporated to dryness; and, after having been strongly heated, to volatilise part of the sal ammoniac, it was redissolved in water, and made to boil. On adding some potash a light black precipitate was obtained, which was carefully collected. This precipitate, while still wet, was dissolved in muriatic acid. The solution, which was yellow, was diluted with a pretty large quantity of water, and decomposed by saturated carbonate of potash, which occasioned some light flocks, that could not be collected without great trouble. The colour of these flocks was a greenish white; and they dissolved in ammonia,

nia, which they tinged blue. This blue solution being evaporated, a little oxide was left, which could not be weighed; but it was treated with muriatic acid, and a slip of iron immersed in the solution, which did not become coated with copper. It is evident therefore, that this small quantity of matter was nickel. The liquor containing saturated carbonate of potash had retained a little manganese, but not the slightest trace of magnesia.

Nickel.

Manganese.

No magnesia.

I boiled the brown precipitate formed by ammonia in a solution of pure potash, which took up some alumine. This was separated, and treated with sulphuric acid. The alumine still retained a small quantity of silex and lime.

Alumine.

After having let the oxide of iron dry, I treated it with muriatic acid, and evaporated to dryness; when a small portion of silex again separated from it. All the silex was mixed together, and heated red hot. The muriatic solution of iron was decomposed by saturated carbonate of potash, filtered, and evaporated; when it still yielded a little oxide of manganese, without any magnesia being discoverable.

Silex.

More manganese, but no magnesia.

As I suspected, that the oxide of iron might still retain a little lime, or magnesia, I redissolved it in muriatic acid, and precipitated by oxalate of ammonia; but I obtained only a light yellow precipitate, which was oxalate of iron mixed with a little oxalate of lime. This I heated red hot, and then redissolved in muriatic acid. This solution I decomposed by ammonia, to get the oxide of iron; and precipitated the lime by oxalic acid. To precipitate all the iron from the solution to which oxalate of ammonia had been added, I employed the hidrosulphuret of ammonia, which formed a black precipitate. This was well washed, dried, calcined, then redissolved in muriatic acid, and precipitated afresh by ammonia. The solution decomposed by hidrosulphuret of ammonia contained no lime.

Iron and a little lime.

It follows from all the facts exhibited in this analysis, that the aerolite of Stannern contains silex, alumine, lime, iron, manganese, nickel, and sulphur; but I found neither magnesia nor chrome.

Component parts, according to Vauquelin.

The

The following are the proportions obtained from 100 parts.

Silex	50
Lime	12
Alumine	9
Oxide of iron	29
Oxide of manganese	1
Oxide of nickel, a slight trace, scarcely to be estimated at ·001	
Sulphur, an atom	
	101.

Muriatic acid found in it.

In another examination of this stone pretty evident traces of muriatic acid were found.

Differences in the two results.

These results differ a little from those obtained by Mr. Moser: 1st, in exhibiting no magnesia; 2dly, in containing nickel; 3dly, in a slight increase of weight, instead of loss; an increase necessarily consequent to the oxidation of the iron. This increase would have been still greater, if I could have calculated the quantity of sulphur disengaged by means of the hidrogen.

Differs from other aerolites,

This aerolite then is of a different species from those that have hitherto been analysed, since it contains neither magnesia, nor chrome, substances constantly found in the other aerolites; and in containing a considerable quantity of alumine, traces of which alone have been found in others.

yet certainly one.

Yet this Moravian aerolite has all the external characters, that distinguish productions of this sort; and from the account given me it appears unquestionable, that it fell from the atmosphere.

Existence of alumine in meteoric stones doubted,

Note. It appears from a former paper in the Annales de Chimie, vol. LXIX, p. 280, that the Institute had entertained some doubts of the reality of the existence of alumine in meteoric stones, announced by Mr. Sage; see Journal, vol. XXIV, p. 190; because it had not occurred in the stones analysed by others. Mr. Vauquelin accordingly analysed a stone, that had fallen no long time before near Parma, but "could not discover in it at most above 0·0015 of alumine." He would willingly have analysed a specimen

specimen of the stone, in which Mr. Sage had found it; but this gentleman had none left, except the piece fashioned into a vase. Mr. Vauquelin however saw the products obtained by Mr. Sage, and was induced to think, that what he had considered as alum was a mixture of the sulphates of alumine, iron, and nickel. The subsequent analysis by Mr. Vauquelin however, given above, not only confirms the existence of alumine in these stones; but that, as Mr. Sage observed, it is in very different proportions; for the stone of Stannern contained more than either of the stones analysed by Mr. Sage. In the following extract of a letter from Mr. Klaproth we find further confirmation of the fact. C.

but now confirmed.

VIII.

Extract of a Letter from Mr. KLAPROTH *to Mr.* GEHLEN *.

I HAVE just finished the analysis of a meteoric stone, that fell in the afternoon of the 13th of May, 1807, in the district of Juchnow, in the government of Smolenski, during a very heavy thunderstorm with a cloudy sky. It weighed 4 poods, or 120lbs. Berlin weight [124lbs. avoir.]. Like all meteoric stones it is covered with a slight grayish black crust. Interiorly it is of an earthy ashen gray. It is mixed with many little specks of pyrites, globules of iron, and many spots of brown oxide of iron. Its specific gravity is 3·7. It gave me as its component parts:

Analysis of another meteoric stone.

Metallic iron	17·60
Metallic nickel	0·40
Silex	38
Magnesia	14·25
Alumine	1
Lime	0·75
Oxide of iron	25
Loss, including a little sulphur, and a trace of manganese	3
	100.

* Annales de Chimie, vol. LXX, p. 185.

It

It is somewhat remarkable, that I found alumine and lime in this meteoric stone, since no mention has been made of alumine in particular in the analysis of meteoric stones hitherto published. It is true Bartholdi, of Colmar, announced 0·17 of alumine in the stone of Ensisheim; but Vauquelin assures us, that he found this stone like all others.

That of Ensisheim contains alumine.

As I have a specimen of the stone of Ensisheim in my collection of meteorolites, I subjected a certain quantity to analysis, and found in it 1½ per cent of alumine.

Alumine in small quantity easily overlooked.

Terre verte.

It is, very easy however, to overlook alumine, when it is contained in very small quantity in stones, as you justly observed on my analysis of the *terre verte* of Verona. Accordingly on repeating this analysis I found, that, by boiling the recently precipitated oxide of iron with a solution of caustic potash, I obtained 1·75 of alumine: this therefore is a principle to be added to those of the earth of Verona, and its weight must be deducted from that assigned to the oxide of iron.

Analyses seldom exact.

To speak generally I am far from pretending, that my analyses, even the most accurate, are perfectly just and exact; it is only by a careful repetition of them, that we can approach as near as possible to the truth.

Decomposition of the alkalis.

Tellurium will probably decompose water by attracting its hidrogen.

Among all the experiments of Ritter on Davy's decomposition of the alkalis I found the phenomena exhibited by tellurium most interesting. [See Journal, vol. XXIV, p. 318.] This property of tellurium leads us to expect, that we may accomplish the decomposition of water so that the hidrogen shall enter into combination, and the oxigen be separated in the form of gas. It is much to be wished, that this metal was more easy to be procured, that we might pursue these experiments; for at present we have no way of obtaining it but from the foliated ore of Nagyag.

Alkaline metals keep well under oil of turpentine, but convert it to a soap.

The products of soda and potash, which we obtained in the laboratory of the academy, in conjunction with Mr. Simon and Mr. Ermann, by employing the galvanic apparatus, have kept very well for three months under oil of turpentine, retaining their metallic lustre, particularly the large pieces. The oil of turpentine has acquired a transparent brown red colour,

lour, and a gelatinous consistence. It is a true Starkey's soap. When I poured fresh oil of turpentine on this soap to dilute it, and exposed it to the action of heat, there was a considerable extrication of gas, and the globules perceptibly diminished. This was owing to the hidrogen, which the soda had absorbed during the galvanic action, being separated again. When these small globules were taken out, and dried on paper, they changed their nature in a few seconds, and nothing was found but small heaps of soda.

IX.

On the Purification of Nickel by Means of Sulphuretted Hidrogen: by Mr. ROBIQUET, *Apothecary**.

HAVING treated a considerable quantity of speiss in the laboratory of Mr. Vauquelin, with a view to extract from it pure nickel, this process afforded me an opportunity of making some remarks, which I have thought necessary to make public, that they who engage in a similar pursuit may avoid the same errours. I shall give a brief account of the process I followed.

Attempt to obtain pure nickel from mispickel.

I took two pounds of speiss, which I treated directly with nitric acid diluted with two parts of water. I omitted the roasting commonly directed, from the danger of so large a quantity of arsenical vapour, and because the roasted ore is not acted upon so easily by acids. Thus the solution, which took place very quickly, was accompanied as usual with an evolution of nitrous gas; and the sulphur separated in proportion as it went on in a flocculent precipitate, which afterward united into a mass. The whole of the acid at 32° employed successively was 6lbs. 8oz.; and the residuum, which probably contained a pretty large quantity of arseniate of iron, weighed 7 oz. 6 drachms. I filtered the liquor, which I diluted with a large quantity of water in

Treated with nitric acid, without previous roasting.

Arseniates of iron and cobalt

* Annales de Chimie, vol. LXIX, p 285.

order

precipitated by potash.

order to dispose the arseniates of iron and cobalt to separate more fully; and as these two salts require more acid, to retain them in solution, than the arseniate of nickel, I accomplished their separation pretty easily, by adding gradually a solution of potash, but so as to leave an excess of acid in the liquor. The precipitate, that was formed, appeared to the eye to contain two substances: the lower stratum was composed of granular rosecoloured particles, which indicated arseniate of cobalt; the other, which was much less in quantity, consisted of finer particles, more flocculent, and of a dirty white. This I considered as arseniate of iron, but I found in it also some arseniate of copper. These two arseniates united and dried weighed ten ounces.

The solution treated with sulphuretted hidrogen.

The solution was treated with sulphuretted hidrogen, as directed by Mr. Proust, for the purpose of separating the copper, oxide of arsenic, and arsenic acid. In fact I obtained so copious a precipitate, that at the end of some days I was obliged to filter, because the tube, through which the gas was introduced, though of considerable diameter, was perpetually getting choked up. I continued to keep up the stream of sulphuretted hidrogen gas, as long as it occasioned any sediment in the solution.

Large quantity of precipitate.

This operation, which continued near a month, and consumed 6 lbs. 8 oz. of sulphuret of iron, yielded me on the whole 1 lb. 14 oz. of precipitate, which I separated at three different periods. The first was of a brown gray colour, and when dried was intermixed with spots of white; the second was more homogeneous, and deeper coloured; and the last was nearly black.

Treated with muriatic acid.

The quantity of these different sulphurets I had obtained appeared to me so exorbitant in respect to the speiss employed, that I was desirous of ascertaining their nature, in order to know, whether I had really separated nothing but copper and arsenic by this process. Accordingly I treated a portion of the first with muriatic acid. The solution was readily accomplished, and the sulphur separated. The filtered liquor was of a fine green, but precipitated white by prussiate of potash. Sulphuretted hidrogen did not render it turbid till after the lapse of a few moments. Having separated the arsenic by this means, I precipitated the whole

whole of the solution with caustic potash. I then treated the precipitate, after it was well washed, with dilute ammonia, the proportion of which I increased, as long as the sapphire blue colour produced was increased in intensity. After macerating some time, I filtered. The undissolved portion had acquired a deep green colour, similar to the fine green of Scheele; and being treated with weak muriatic acid, it immediately became black, and dissolved, giving out oximuriatic acid gas, and imparting to the acid a fine pure rose colour. It was therefore oxide of cobalt at a maximum, which could not redissolve in the ammonia, as Mr. Thenard had observed. The ammoniacal solution, evaporated to dryness, let fall a light green powder, which comported itself as pure oxide of nickel with every reagent.

Precipitate treated with ammonia.

Residuum dissolved in muriatic acid.

On the successive examination of the other two precipitates obtained by sulphuretted hidrogen, they were found to agree, except in the proportions, with the former. In the last the quantity of cobalt was scarcely perceptible. The second contained more than a tenth of arsenic in the state of an oxide not sulphuretted, which might be extracted by boiling water alone. The first contained much more nickel than the others. By the different processes which I employed to analyse the precipitates I constantly found sulphur, originating from the sulphuretted hidrogen, arsenic, nickel, cobalt, and not an atom of copper. This did not surprise me, because I knew, that some of these ores contain none: but what really puzzled me was, to account for the precipitation of the nickel and cobalt by the sulphuretted hidrogen. I thought at first this was owing to the process I had followed; but I was completely undeceived, after having analysed three specimens taken from different collections, and labelled "sulphuret of copper and arsenic obtained in the purification of nickel." These sulphurets were of the same nature as mine, only one of them contained in addition a little copper; yet they were produced by processes, in which alkali had not been employed for the separation of the arseniates of iron and cobalt as in mine. Obliged to look for some other source of the fact, I formed a series of conjectures, and made experiments accordingly.

Precipitates at different periods differed only in proportions.

No copper.

What could occasion the precipitation of the nickel and cobalt by the sulphuretted hidrogen?

cordingly, which it would be useless to enumerate, since they led to nothing interesting.

Not the addition of some foreign substance.

I could not suspect the existence of a substance, which, when united with nickel and cobalt, occasions their precipitation by sulphuretted hidrogen: for, when I redissolved these sulphurets in muriatic acid* mingled with a few drops of nitric acid, and I had separated from them nothing but sulphur, I could no longer precipitate them again by the same reagent. It must have arisen therefore from some circumstances peculiar to the operation itself: and in consequence, in order to examine it with more attention, I began with a small quantity. The solution was accomplished in the same way by nitric acid diluted with twice its quantity of water by measure. The residual sulphur was separated by filtration, and I evaporated the solution gently to one fourth. I let it cool, to crystallize the oxide of arsenic, which was deposited in pretty large quantity. Having filtered the solution a second time, I evaporated it to a sirupy consistence: when in proportion as the excess of acid was carried off, the arseniate of iron was deposited, and formed a kind of gelatinous magma. Again I filtered and evaporated: and this I repeated to the fourth time, drawing off at each a little more acid.

Experiment with a smaller quantity.

Nitric solution evaporated and filtered repeatedly.

Different precipitates.

The first sediment obtained by evaporation was arseniate of iron in the form of a white powder: the second was a mixture of arseniate of cobalt and iron: the third appeared to be pure arseniate of cobalt. The last time I evaporated to dryness; and the portion that would not redissolve in water was arseniate of nickel.

Sulphuretted hidrogen added to the solutions.

At each filtration I took care to try the fresh solution with sulphuretted hidrogen, to endeavour to find the point at which the precipitation of the nickel took place; but I always obtained a precipitation of orpiment. The last alone yielded me a copious sediment of a blackish brown, which was found on examination to contain nickel, arsenic, and no copper. I then conceived, that the proportion of acid might have an influence in this precipitation. With this view I

Solution still acid.

* I perceived, that, during this solution, the muriatic acid took up orpiment in a state of combination, which could be separated from it by the addition of water.

examined

examined the state of my solution, and I saw, that it still reddened litmus paper perceptibly; and if I added a small portion of acid, the precipitate produced in it by sulphuretted hidrogen was no longer black, but of a fine yellow.

Thus it appears to me demonstrated, that when the nitric acid is in too small proportion to exert a strong attraction for the oxide of nickel, it lets fall a certain quantity with the arsenic, till it becomes sufficiently predominant to counterbalance the combined action of the sulphur and arsenic on the nickel. Whence we perceive, that, to avoid this inconvenience, it is necessary to keep up a slight excess of acid in the liquor, and then we may operate with certainty.

A predominance of acid necessary, to prevent the precipitation of the nickel.

X.

On the Experiments of Mr. Chenevix *and Mr.* Descotils *on Platina: by Mr.* C. L. Berthollet*.

Mr. Chenevix published in the Philosophical Transactions a number of experiments on the combination of platina with mercury†, from which it appeared, that this compound, in certain proportions, was capable of sustaining the strongest fire without the mercury being separated; that it was fusible; and that it bore a resemblance to palladium. But that celebrated chemist was desirous of his experiments being repeated and confirmed, particularly as Messrs Rose and Gehlen announced results contradictory to his. Accordingly I invited him to accompany Mr. Descotils to my laboratory at Arcueil.

Mr. Chenevix combined mercury with platina.

Mr. Chenevix mentioned the following experiment, which he has described in the Annales de Chimie, vol. LXVI, p. 86, as best calculated to elucidate the subject.

Experiment pointed out by him.

" Let diluted nitric acid be boiled with a large quantity of metallic mercury, and the result will be a nitrate of mercury at a minimum of oxidizement. If this be poured into a mu-

Nitrate of mercury at a minimum added to muriate

* Annales de Chimie, vol. LXVII, p. 86.

† See Journal, vol. VII, p. 85, 176; and XI, p. 162 and 182.

of platina, and precipitate reduced with a little borax and charcoal.

a muriatic solution of platina, a precipitate will be formed, composed of platina and mercury united with muriatic acid. Let this be washed, and reduced with a little borax in a crucible lined with charcoal, and a metallic button will be obtained. Dissolve this in nitro-muriatic acid, and it will be precipitated by green sulphate of iron."

These directions were followed, and a well melted button was obtained, the specific gravity of which was about 17.

Platina precipitated by green sulphate of iron.

Mr. Descotils having subjected to ebullition a mixture of green sulphate of iron and solution of platina, prepared so as to contain but little excess of acid, a copious precipitate fell down; so that this property belongs to platina itself.

Another difference remained; it was, that the platina precipitated with mercury possessed the property of being liquefiable by the action of fire, which indicated a considerable difference between the button obtained and platina that had not undergone the same preparation.

Platina fusible with the addition of borax.

In the interval between this meeting and the next, Mr. Descotils alone treated pure platina with borax in a crucible lined with lampblack, and obtained a button like that given by the mixed precipitate, and of similar specific gravity.

The button contained boracic acid.

By this experiment, which was repeated, it was proved, that platina is capable of entering into fusion by means of borax and charcoal. On dissolving part of the button, Mr. Descotils obtained from it boracic acid.

Platina combined with borax.

Platina therefore is capable of combining with the whole or part of the borax; and thus alloyed it has a perfectly metallic aspect, is hard and very brittle, and takes a crystalline form internally. Mr. Descotils had already observed similar phenomena with other metals.

Precipitate of platina and mercury not fusible without charcoal and borax.

The mixed precipitate urged with the most powerful forge fire, without the addition of charcoal and borax, did not enter into fusion; so that Mr. Chenevix acknowledged the property of liquefying was not owing to the mercury, with which the platina had been precipitated, but to the charcoal and borax; and he had himself remarked in his former paper, that platina might be fused by means of borax, in a crucible lined with charcoal no doubt, as will presently be seen.

There

There were two substances, the borax and charcoal, the action of which it was necessary to discriminate. In consequence Mr. Descotils treated pure platina with charcoal in a crucible, and observed, that the charcoal was sufficient, to produce its fusion.

Platina fused with charcoal alone.

A button thus obtained at Arcueil had increased in weight a little more than 0·03, which were owing to the charcoal, that had combined with the metal. Its specific gravity was 18. This carburet was very hard, very brittle, shining, and lamellated internally.

Carburet of platina.

Mr. Descotils observed, that borax without charcoal could not effect the liquefaction of platina; yet, by means of charcoal, borax, or more probably the boracic acid, enters into combination with platina. He will publish the experiments, to which these observations shall give rise, and which he intends to pursue.

Borax alone will not fuse platina.

XI.

On a singular Crystallization of the Diamond: by Mr. GUYTON-MORVEAU*.

THE natural history of minerals being only a collection of facts sufficiently established to be admitted without question, it is particularly important to confirm the reality of those that rarely meet our observation, in order to remove doubts, and cut short disputes, which tend but to retard the progress of science.

Natural history of minerals a collection of facts.

Five years ago I published in the Annales de Chimie, a letter on the crystallization of lazulite, the lapis lazuli of the shops. The piece from which the accurate description was then made by Lermina, and which presents a perfect dodecaedron with rhombic faces, has been recognized by all who have seen it in my collection with the fine ultramarine, which Messrs. Desormes and Clement obtained from the mass in which this crystal was imbedded. Their opinion

Crystallization of lapis lazuli.

* Read to the Institute, March the 20th, 1809. Ann. de Chim. vol. LXX, p. 60.

has

has been unanimously confirmed by comparing it with another fragment, in which likewise are found a number of grains of lazulite, enchased as it were in carbonate of lime, equally accompanied with sulphuret of iron, all of the same azure blue colour, and some of which pretty distinctly exhibit angles and faces, notwithstanding their smallness.

Some have unreasonably doubted this.

Most of the mineralogists who have written since this publication have made no scruple to mention this regular figure under the article lazulite, though others have not even attempted to fix their opinion by an examination of the crystal, a description of which I have published. They have concluded, that, being opake and mixed with pyrites and carbonate of lime, there was no proof of its being really a lazulite; notwithstanding they agree with all other mineralogists, that the fossile body which yields ultramarine is always opake, and commonly mixed with pyrites and carbonate of lime.

New crystal of the diamond.

Having had an opportunity of observing a crystalline form of the diamond perhaps hitherto unique, I conceived the Class would receive some gratification from seeing it, and that this would be the most certain method of establishing its existence beyond all controversy.

Usual form of its crystals.

The primitive form of the diamond is known to be a regular octaedron. Most frequently it presents itself in spheroidal crystals or with curvilinear facets. It has been found cubical, plano-convex, cylindroid; but it was not suspected to be susceptible of that variety of form, which Romé de l'Isle termed *macle*, and Mr. Haüy has named *hemitrope*; that is, where half of the crystal is turned back, so as to form reentering angles, as we see in some varieties of the ruby, feldspar, pyroxene, &c.

A macle or twin crystal.

Among the rough diamonds, which Mr. d'Arcet offered to sacrifice to the series of experiments undertaken by Messrs. Hachette, Clement, and myself, on the products of their combustion, there was one, which we thought proper to set aside, as presenting the first example of a structure, the prototype of which ought to be preserved.

This diamond is of the weight of 702 millig. [11 grs. nearly]. Its specific gravity is 3·512.

The class will perceive at once, that it is formed of two demispheroids,

demisphenoids, the deflected position of which, imperfectly terminated at one of the extremities, exhibits at the other the very decided reentering angles, that characterise the hemitrope.

XII.

Remarks on the Pechblende, an Ore [illegible]: *by Mr.* VAUQUELIN*.

Mineral resembling gadolinite, or pechblende,

MR. de Launay, a dealer in minerals, and a skilful connoisseur in this branch of natural history, had in his collection several specimens of a black, shining, compact, heavy mineral, with a conchoidal fracture, the nature of which he did not know. Mr. Haüy, to whom he showed it, was in doubt between gadolinite and pechblende, which in fact it resembled more than any other substance.

proved to be the latter.

Having given me a piece of it to subject to chemical analysis, I soon found, that it was pechblende, or sulphuret of uranium. Though the experiments, by which I found what it was, have nothing new in themselves, I shall relate some of them, because they afforded me an opportunity of making a few remarks on the state in which uranium exists in this ore, and on the combinations it is capable of forming with oxigen.

Treated with muriatic acid.

The pechblende reduced to powder, and digested in muriatic acid moderately concentrated, dissolved without any perceptible effervescence, only a smell of sulphuretted hidrogen gas was emitted.

Solution filtered,

The solution had a very deep dirty green colour. Diluted with water and filtered, it left on the paper a small quantity of residuum; which, when washed and dried, ex-[illegible] all the properties of silex mixed with a little sulphur.

& evaporated.

During the evaporation, and more especially during the cooling, this concentrated solution deposited crystals of muriate of lead. These I separated by adding rectified alcohol,

* Annales de Chimie, vol. LXVIII, p. 277.

hol, which dissolved the muriate of uranium without touching the muriate of lead.

The muriate of uranium dissolved in water and examined.

The muriate of uranium, after the alcohol was driven off by heat, was diluted with water, and subjected to the following experiments. 1. Pure and carbonated alkalis formed in this solution very deep bottle-green precipitates. These precipitates, particularly that from ammonia, turned black and shining when dried. 2. Prussiate of potash produced a deep chocolate brown precipitate. 3. Infusion of galls occasioned a greenish brown sediment which in time changed of a yellowish red in the upper part.

Precipitate by ammonia,

The precipitate formed by ammonia in the muriatic solution of uranium, though completely washed and dried in the open air, still retains much water and ammonia; for on

calcined,

being heated in a glass retort, it gave out a perceptible quantity of these two substances, and acquired a still deeper black colour.

and dissolved in nitric acid.

This matter, thus dried, dissolved readily in nitric acid diluted with water, even cold: but the solution, which had a green colour, immediately on being heated gave out a considerable quantity of nitrous vapours, and at the same time acquired an orange yellow colour.

The solution diluted,

The solution of this calcined oxide in nitric acid having been diluted with water, after some time it let fall a small quantity of oxide of iron. This ore of uranium therefore contains small quantities of lead, iron, sulphur, and silex. It appears to be the kind analysed by Mr. Klaproth under the name of pechblende of Joachimsthal.

& precipitated by alkalis,

This new solution of oxide of uranium in nitric acid was precipitated of an orange yellow by caustic alkalis, and of a pale yellow by alkaline carbonates, an excess of which redissolved the precipitate.

prussiate of potash, & galls.

Prussiate of potash and infusion of galls formed in this solution precipitates of a brown red much lighter than before.

Green oxide dissolved in muriatic acid,

The green oxide of uranium united with liquid oximuriatic acid soon destroyed its smell and colour; and the solution [illegible] yellow colour, like that effected by [illegible].

and in other acids, crystallized.

The combinations of the green oxide of uranium with the sulphuric, [illegible], and acetic acids, evaporated

to

to a proper degree, afford neutral salts, which crystallize each in its peculiar form, but all of a yellowish green colour.

The yellow oxide on the contrary combined with these acids never forms perfectly neutral salts, and does not crystallize in distinct and regular figures.

Salts of the yellow neither neutral nor crystallizable.

The green oxide does not dissolve in alkaline carbonates: but the yellow oxide does in large quantity, as Mr. Klaproth first observed.

The yellow dissolves in alkalis, but the green does not.

From these experiments it appears to me beyond a doubt, that uranium is susceptible of two degrees of oxidation, and this is principally the object I had in view to demonstrate; the one by which it forms a deep green oxide, the other, by which it yields an orange yellow oxide. It exists in the first of these states in the pechblende; and it is in the second that it constitutes the yellow ore of uranium, such as that which Mr. Champeux discovered in the environs of Autun, in the department of the Upper Saone.

Two oxides of uranium therefore.

The pechblende of which I have given the analysis appears to me to contain uranium in the first degree of oxidation, for it dissolves in muriatic acid without any perceptible evolution of gas.

Oxide of the first degree in pechblende,

I even doubt whether the sulphur contained in this ore be combined with the oxide of uranium, for the quantity is extremely small in proportion to that of this metal; and am more inclined to suppose that it belongs to the lead, which also exists in it.

which is not a sulphuret.

This reflection did not escape Mr. Klaproth, for he says in the second volume of his analyses: "I do not consider the black ore of uranium as a sulphuret, but as a metal combined with little oxigen. It is this nearly metallic state of the ore that occasions the evolution of nitrous gas while it is dissolving in nitric acid." Mr. Klaproth however has not distinguished two species of the oxide.

Klaproth of a similar opinion.

These two oxides comport themselves with the acids and alkalis nearly as the oxides of iron. Thus the green oxide of iron uniting with acids saturates them perfectly, and forms crystallizable salts, and does not dissolve in alkalis. The red oxide on the contrary, in its combination with acids, which it does not saturate, forms uncrystallizable salts; and it dissolves in concentrated subcarbonates.

The oxides analogous to those of iron

From

Capable of forming hidrurets.

From the experiments above related we may farther conclude, that the oxides of uranium, like several other metals, are capable of combining with water and forming hidrurets; a fact which still farther confirms the opinion of Mr. Proust on this subject.

XIII.

On the Oxides of Copper, by Professor Proust *.

Oxides of copper.

I Have just analysed red oxide of copper crystallized in octaedra. I have found, that it is an oxide at a minimum, and obtained from it

Red.

Copper··100	.... or	84·75
Oxigen·· 17 or 18	....	15·25

The black oxide of copper contains

Black.

Copper··100	.... or	80
Oxigen·· 25	....	20.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Wernerian natural history Society.

THE first meeting of the third session of this Society was held in the College Museum at Edinburgh, on the 4th of November last. There were then read a learned botanical paper, by Mr. R. Brown of London; proposing a subdivision of the apocineæ of Jussieu, to be called asclepiadeæ; the first part of an essay on Meteoric Stones, by Mr. G. J. Hamilton; and the concluding part of an account of Fishes found in the Frith of Forth, by Mr. Neill.

The next meeting was on the 9th of December, when Professor Jameson read an account of a considerable number of animals of the class vermes, which he had observed on the shores of the Frith of Forth, and the coasts of the Orkney and Shetland Islands; and also a series of observa-

Gems found in Scotland.

* Journal de Physique, vol. LXV, p. 60.

tions

tions on the different precious stones found in Scotland, particularly the topaz, of which he exhibited a series of interesting specimens from Aberdeenshire; and among these was a crystal, weighing nearly eight ounces, which is probably the largest crystallized specimen hitherto discovered in any country. The secretary laid before the meeting a communication from the Rev. Mr. Fleming of Bressay, describing several rare vermes lately discovered by him in Shetland; and a catalogue of rare plants to be found within a day's excursion from Edinburgh, by Mr. Robert Maughan, senior.

Large crystallized topaz.

At this meeting the following gentlemen were chosen office-bearers for 1810:—Prof. Jameson, *President*: Drs. Wright, Macknight, Barclay, and T. Thomson, *Vice-Predents*:—P. Walker, Esq. *Treasurer*:—P. Neill, Esq. *Secretary*:—P. Sime, Esq. *Painter of Objects in natural history.*

We understand that the first volume of the Transactions of the Wernerian Natural History Society is in the press, and will appear early in the ensuing year; and also that Dr. Charles Anderson of Leith, the learned translator of Werner's classical work on veins, has now in the press a translation of the celebrated Von Buch's Mineralogical Description of the county of Landen in Silesia.

1st vol. of Wernerian Transactions.

Daubuisson, a distinguished pupil of the illustrious Werner, some time ago published an excellent description of the Floetz-trap formation of Bohemia; and it gives us pleasure to announce, that a translation of this work, by a member of the Wernerian Society, is nearly finished, and will appear early in the ensuing spring.

Translation of Daubuisson's Floetztrap Formation in Bohemia.

Werner has had the distinguished honour conferred on him, of being elected one of the Honorary Fellows of the Royal Society of Edinburgh; and also Honorary Member of the Royal Medical, Royal Physical, Natural History, and Chemical Societies of Edinburgh, and of the Literary and Philosophical Society of Manchester.

Werner elected fellow of various societies.

A Caledonian Horticultural Society has recently been established at Edinburgh, nearly on the plan of the Horticultural Society of London. It is to consist of limited numbers of honorary, ordinary, and corresponding members.

Horticultural Society in Scotland.

cbers. It is intended, that the Society shall be very select, consisting only of persons distinguished for their horticultural and botanical zeal. The Society purposes to publish Memoirs; and we doubt not that much useful information may thus be disseminated. The following gentlemen have been chosen office-bearers for 1810.

President. The Right Hon. the Earl of DALKEITH. Vice-Presidents. Sir JAMES HALL, Bart., M.P. Dr. RUTHERFORD, Prof. of Botany, Edin. Dr. COVENTRY, Prof. of Agriculture, Edin. ALEX. G. HUNTER, of Blackness. Mr. WALTER NICOL, and Mr. PATRICK NEILL, Secretaries. Mr. ANDREW DICKSON, Treasurer. Counsellors. Professional. Mr. THOMAS DICKSON, *Leith Walk*. Mr. JAMES MACDONALD, *Dalkeith*. Mr. EDWARD SANG, *Kirkaldy*. Mr. THOMAS SOMMERVILLE, *Botanic Garden*. Mr. JOHN FLETCHER, *Restalrig*. Mr. JOHN HAY, *Edinburgh*. Amateurs. Dr. DUNCAN, Sen. Dr. JAMES HOME. R. HODSHON CAY, Esq. GEORGE BRUCE, Esq. THOMAS HUTCHISON, Esq. JAMES SMITH, Esq.

Mathematical Repository.

Mr. T. Leybourn, of the Royal Military College, has just published the ninth number of his periodical work, entitled, The Mathematical Repository. It contains, beside various articles, Solutions to the Mathematical Questions proposed in the seventh number; and a series of New Questions, to which he solicits Answers from his correspondents, with a view to their being inserted in the eleventh number.—In publishing this work, the editor has in view, to promote the study of the various branches of the mathematics, by affording to the student an opportunity of cultivating his powers of invention in resolving problems which depend on its different theories; and also, to collect together and preserve the fruits of the studies of his ingenious correspondents, among whom he numbers some of the most skilful mathematicians in this country. The number here announced completes the second volume of the work. The many valuable articles in both these we would gladly enumerate, but that they are so numerous, they would occupy too much room.

Meteoric stone of Weston.

Prof. Woodhouse has analysed the meteoric stones that fell at Weston, in Connecticut, on the 14th of October, 1807,

1807, and obtained from 100 parts, silex 50, iron 27, sulphur 7, magnesia 10, nickel 1, leaving a loss of 5. Some specimens of it, carried to France by col. Gibbs, were examined by Mr. Gillet-Laumont, who gives the following account of them.

"They contained rounded globules, ferruginous and brittle, of a blackish gray, and acquiring a dull metallic aspect on being rubbed with a smooth file. They were not very abundant, and appeared to be slightly attracted by the magnet.

Contained brittle iron,

"Small portions of malleable iron were diffused very plentifully through the stones. They were of irregular shapes, and very unequal in size; some black, but most of a shining silvery white; and easily cut with a steel instrument, like those contained in most aerolites. I separated a small, flat, triangular piece, about a quarter of an inch long, which I heated to different degrees, and afterward plunged into cold water, but could not make it harder.

malleable iron,

"On the face of one of the stones were some particles of mica; but as I could find none interiorly, I suppose they came from the ground on which it fell.

"Another of the specimens contained embedded in it a portion of a small body of the size of a pea, of a whitish gray colour, composed of smooth, shining lamellar facets, forming angles too small to be measured. It resembled a piece of broken feldspar. On endeavouring to detach a piece for the purpose of assaying it, the small mass immediately separated, leaving a cavity, which showed, that it was rounded before it was moulded in the stone. A particle of a very similar substance still exists in the stone; and there are some yellowish particles in the cavity from which this lamellar substance was taken.

and a lamellar substance.

"This substance scratched German sheet glass. It did not effervesce with nitric acid. Heated before the blowpipe it was immediately covered with a black enamel, which transuded in small globules, but the mass did not melt. I should have examined it farther, but I let it fall, and could not again find it.

Described.

"The aerolite of Weston therefore contained a substance, which was neither carbonate of lime nor feldspar, and I believe

Neither carbonate of lime nor feldspar.

lieve it is the first time that a lamellar substance, having the true elements of crystallization, has been mentioned as found in a stone fallen from the atmosphere."

Skeleton of the mammoth.

The skeleton of the mammoth found in the ice at the mouth of the Lena [See Journ. vol. XIX, p. 158], which has been for some time publicly exhibited at Moscow, is said to be intended for the museum of the Imperial Academy of Sciences at Petersburg. Prof. Tilesius has made forty drawings of the skeleton and its various parts, which he means to publish in folio, with observations. On some points he differs from Cuvier.

Cold of last winter.

The greatest cold of last winter observed at Moscow was in the night of the 11th of january. Mercury exposed to the open air in a cup by Dr. Rehmann was frozen so hard, that it could be cut with sheers, and even filed. Count Boutourline found the mercury in three thermometers withdrawn entirely into the ball, and frozen; but in another it was seen by himself, and four other persons from 6 o'Clock till half after at —35° R. [—46¾° F.]. Mr. Rogers, of Troitsk, is said to have seen it at — 34° [— 44½° F.] before it froze, and withdrew into the ball.

Conveyance of sound through solid bodies.

The aqueducts constructing at Paris have enabled Mr. Biot to make experiments on the propagation of sound through solid bodies on a larger scale, than had hitherto been done. The total length of the pipes was 951 met. [3118 feet]. A blow with a hammer at one extremity was heard at the other, producing two distinct sounds, the interval of which, measured in more than 200 trials, was 2·5″. The temperature was 11° [51·8 F.]. According to the experiments of the academy the time of the propagation of sound to this distance through the air should be 2·79″, at this temperature; from which if we deduct 2·5″, the interval observed, we have 0·29″ for the time the sound was in being propagated through the solid substance. This result was confirmed in another way. Two persons were stationed at the opposite extremities of the canal, each furnished with a half-second watch carefully compared, and each struck alternately with the hammer at intervals of 0, 15, 30, and 45 seconds. The time of the arrival of the two sounds was noted; and the sum of the numbers indicated by the watches

watches gave double the time of the propagation by the solid substance, independent of the difference there might be between them. Thus the time of the transmission by the solid was found by repeated observations to be 0·26″, and of that by the air 2·76″. The first result differs from that given by the intervals of the sounds only 0·03″; and the second differs from the time deduced from the observations of the academy just as much: an agreement that appears to confirm the results.

Mr. Biot likewise observed, that at this distance the lowest voice could be heard perfectly from one end to the other, and with sufficient distinctness to keep up a conversation.

Native green oxide of chrome.

Mr. Leschevin, chief commissary of gunpowder and salt-petre, has sent from Dijon to the Council of Mines a collection of specimens of rocks, interesting on account of the parts coloured green that they contain. Several pieces of this stone, and a siliceous breccia, improperly called chalcedony of Creuzot, containing the same substance, had been found in abundance on the road; and Messrs. Guyton and le Lievre had ascertained, that the green colour was not owing to copper; but it was not known whence they came. After much search Mr. Leschevin discovered these green rocks in three contiguous mountains, and found that they were coloured by oxide of chrome, combined in greater or less quantity with silex, alumine, &c. On one of these mountains he met with the graphic granite, which several authors have mentioned as accompanying the emerald; and he intends to search for this stone also, which Mr. Vauquelin has discovered to be sometimes coloured with chrome. Since Mr. Drappier has shown, that chrome united with lead makes the most beautiful of all yellows, this may turn to considerable account.

Electric repulsion in the inverse ratio of the distance simply.

Mr. Simon, of Berlin, has been making some experiments on the true law of electric repulsion. Coulomb appears to have established this law by means of the torsion of wire in his electrical balance, that the electrical repulsion is in the inverse ratio of the square of the distance. To prove this law to his audience by a more simple and firm apparatus, Mr. Simon constructed a pair of scales, all the parts of which were made

made of glass, and coated with gum lac. Though inferior in sensibility to Coulomb's apparatus, it appears sufficiently sensible for experiments of this kind, since each degree of deviation of the tongue of the balance from 0 was equal to the weight of 0·04 of a grain. The result of Mr. Simon's experiments, the circumstances of which he varied in every possible way, was, that the electric repulsion was in the simple inverse ratio of the distance. In trials with the gold leaf electrometer this law was established with still more precision than in those which he made with the pith balls. It is to be observed, that Volta has always denied the truth of Coulomb's law, and asserted, that experiments with the electrophorus show the electric repulsion and attraction to be simply in the inverse ratio of the distance.

Greatest density of water.

Mr. Tralles has found by a very simple experiment, that the temperature at which water possesses the greatest density is at 39·83° F., or 4·35 of the centesimal thermometer; and he conceives this point, being more fixed than that of congelation, should be taken as the 0.

Standard of specific gravities.

Considering pure water at this density too as the proper unit for specific gravities, Mr. Tralles has given proper formulæ and a table for calculating specific gravities from this unit according to the different heights of the barometer and thermometer. Mr. Karsten has adopted these formulæ in calculating the specific gravities of minerals from his own experiments, with more accuracy than has usually been done, for the new edition of his Mineralogical Tables just published.

Answer to Dalton.

In another paper in Gilbert's *Annales der Physick*, entitled "Principles of Areometry, exhibited in the most general manner, and applied to the Vapour of Water, as a Critique on the Hypothesis of Dalton, and on some Calculations of the Density of the Vapour of Water," Mr Tralles demonstrates the insufficiency and erroneousness of Mr. Dalton's hypothesis of the mode in which elastic fluids mix with each other, and the constitution of the atmosphere. From the experiments he quotes the specific gravity of the vapour of water is to that of dry air as 1 to 1·45, in equal circumstances. From the results of the experiments of Biot and Arrago compared with those of Gilpin, he finds the density of dry

Specific gravity of vapour.

air

air under a pressure of 0·76 m. [29·9 inches], and in the latitude of 45°, = 0·00129918 of the density of water at the temperature of melting ice, under equal circumstances, and = 0·0012770 of water at its greatest density. The specific gravity of mercury is 13·59925 at the former standard, and 13·59655 at the latter.

dry air, and mercury.

The plant that furnishes gum ammoniacum is not known, but it was supposed to be of the umbelliferous kind from the seeds frequently found mixed with it. Mr. Willdenouw however has been so fortunate as to get the seeds to germinate, and of the plant produced he has made a new genus, calling it *heracleum gummiferum*. As the root of this plant however contains no milky juice, he thinks we cannot yet consider it as certainly the plant that produces the ammoniacum.

Plant that produces gum ammoniacum.

Mr. Suersen has given the following mode of preparing benzoic acid. Boil four ounces of benzoin in powder with three drachms of carbonate of soda in a sufficient quantity of water for an hour. Take out the benzoin, powder it afresh, and boil it again for half an hour in the same liquor. After several alternate boilings and powderings the soda will be entirely saturated with it, and five drachms of very pure benzoic acid may be precipitated from it by means of sulphuric acid; which is at the rate of 2½ ounces from a pound, or nearly one sixth.

Mode of preparing benzoic acid.

Mr. Rose had observed, that castor oil was completely soluble in alcohol. Mr. Bucholz has confirmed this, and says they unite in any proportion. Hence its sophistication with any fat oil may readily be detected; for these, though not completely insoluble in alcohol, do not mix with it except in very small quantities. Thus 60 drops of alcohol dissolve 2 drops of oil of almonds, 2 of poppy oil, 1 of rape oil, and 3 of old linseed oil. With the assistance of heat they would dissolve more.

Castor oil soluble in alcohol.

Dr. Buxton's spring course of Lectures on the Theory and Practice of Medicine will be commenced about the middle of January, 1810, at the Medical Theatre, London Hospital.

Medical lectures.

METEOROLOGICAL JOURNAL,

For DECEMBER, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

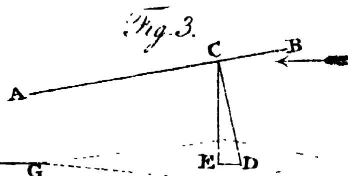
NOV. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
27	42°	38°	43°5	32°	29·63	Fair	Rain
28	34	30	41	32	29·94	Cloudy	Cloudy
29	31	36	41	31	29·88	Ditto	Ditto
30	33	43	44·5	41	29·87	Fair	Fair
DEC.							
1	42	38	45	32	29·30	Ditto	Rain
2	37	37	43	33	29·69	Ditto	Fair
3	38	47*	47·5	43	29·92	Rain	Rain
4	44	41	47	37	29·50	Hail	Cloudy
5	38	43	44·5	42	29·88	Fair	Ditto
6	47	52	53	50	30·02	Cloudy	Ditto
7	†54·5	44	54·5	37	29·84	Rain	Fair
8	39	45	47	42	30·24	Fair	Cloudy
9	45	52	52	44·5	29·66	Cloudy	Rain
10	45	41	46·5	36	29·18	Fair	Ditto
11	38	40	45	37	29·37	Ditto	Fair
12	46	41	46·5	36	28·96	Rain	Cloudy
13	40	36·5	42·5	32	29·20	Ditto	Fair
14	34·5	36	42	36	29·40	Fair	Cloudy
15	40	36·5	43·5	33	28·76	Cloudy	Ditto
16	35·5	41·5	43	38	28·83	Fair	Rain
17	34	42	44	39	28·68	Rain	Cloudy
18	40	44*	44	37·5	28·32	Ditto	Ditto
19	41	42	43	37	29·28	Ditto	Fair
20	39	42·5	44	35	29·75	Fair	Rain
21	36·5	40	43·5	37	29·75	Ditto	Fair
22	39·5	41	44	37	29·95	Ditto	Ditto
23	38	39	41	32	29·90	Ditto	Ditto
24	34·5	35	38·5	31	29·83	Snow	Ditto
25	37	44*	44	36	30.00	Rain	Rain
26	39	41	42	36	29·76	Ditto	Ditto
27	40	41	43	35	29·30	Ditto	Cloudy

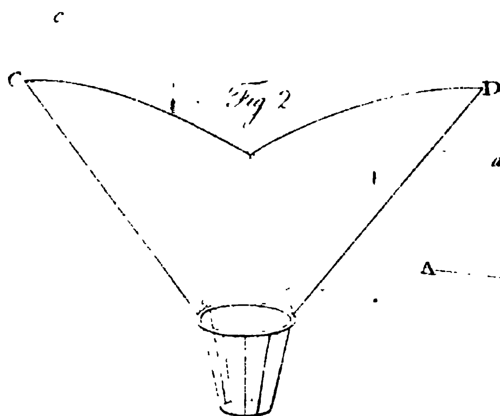
* Maximum of the Thermometer in the evening.

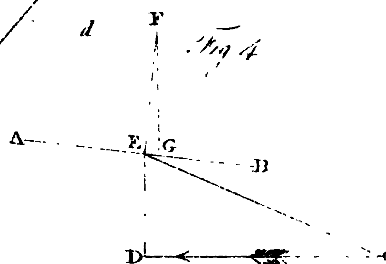
† Maximum of the Thermometer at 9 A. M.

Sir G. Cayley on Aerial Navigation

Fig 1







Leaf of the Mimosa pudica





A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY, 1810.

ARTICLE I.

On Aerial Navigation. By Sir GEORGE CAYLEY, *Bart.*

HAVING, in my former communication*, described the general principle of support in aerial navigation, I shall proceed to show how this principle must be applied, so as to be steady and manageable.

Descent by a parachute.

Several persons have ventured to descend from balloons in what is termed a parachute, which exactly resembles a large umbrella, with a light car suspended by cords underneath it. Mr. Garnerin's descent in one of these machines will be in the recollection of many; and I make the remark for the purpose of alluding to the continued oscillation, or want of steadiness, which is said to have endangered that bold aeronaut. It is very remarkable, that the only machines of this sort, which have been constructed, are nearly of the worst possible form for producing a steady descent, the purpose for which they are intended. To render this

Of the worst possible form for a steady descent.

* See Journal, vol. XXIV, p. 164.

Stability of a boat.

subject more familiar, let us recollect, that in a boat, swimming upon water, its stability or stiffness depends, in general terms, upon the *weight* and distance from the centre of the section elevated above the water, by any given heel of the boat, on one side; and on the *bulk*, and its distance from the centre, which is immersed below the water, on the other side; the combined endeavour of the one to fall, and of the other to swim, produces the desired effect in a well constructed boat. The centre of gravity of the boat being more or less below the centre of suspension is an additional cause of its stability.

Effect of a parachute, as commonly made,

Let us now examine the effect of a parachute represented by A B, Fig. 1, Pl. III. When it has heeled into the position *a b*, the side *a* is become perpendicular to the current, created by the descent, and therefore resists with its greatest power; whereas the side *b* is become more oblique, and of course its resistance is much diminished. In the instance here represented, the angle of the parachute itself is 144°, and it is supposed to heel 18°, the comparative resistance of the side *a* to the side *b*, will be as the square of the line *a*, as radius, to the square of the sine of the angle of *b* with the current; which, being 54 degrees, gives the resistances nearly in the ratio of 1 to 0·67; and this will be reduced to only 0·544, when estimated in a direction perpendicular to the horizon. Hence, so far as this form of the sail or plane is regarded, it operates directly in opposition to the principle of stability; for the side that is required to fall resists much more in its new position, and that which is required to rise resists much less; therefore complete inversion would be the consequence, if it were not for the weight being suspended so very much below the surface, which, counteracting this tendency, converts the effort into a violent oscillation.

and as it ought to be made.

On the contrary, let the surface be applied in the inverted position, as represented at C D, Fig. 2, and suppose it to be heeled to the same angle as before, represented by the dotted lines *c d*. Here the exact reverse of the former instance takes place: for that side, which is required to rise, has gained resistance by its new position, and that which is required to sink has lost it; so that as much power operates

to restore the equilibrium in this case, as tended to destroy it in the other: the operation very much resembling what takes place in the common boat*.

This angular form, with the apex downward, is the chief basis of stability in aerial navigation; but as the sheet which is to suspend the weight attached to it, in its horizontal path through the air, must present a slightly concave surface in a small angle with the current, this principle can only be used in the lateral extension of the sheet; and this most effectually prevents any rolling of the machine from side to side. Hence, the section of the inverted parachute, Fig. 2, may equally well represent the cross section of a sheet for aerial navigation.

Basis of stability in aerial navigation.

The principle of stability in the direction of the path of the machine, must be derived from a different source. Let A B, Fig. 3, be a longitudinal section of a sail, and let C be its centre of resistance, which experiment shows to be considerably more forward than the centre of the sail. Let C D be drawn perpendicular to A B, and let the centre of gravity of the machine be at any point in that line, as at D. Then, if it be projected in a horizontal path with velocity enough to support the weight, the machine will retain its relative position, like a bird in the act of skimming; for, drawing C E perpendicular to the horizon, and D E parallel to it, the line C E will, at some particular moment, represent the supporting power, and likewise its opponent the weight; and the line D E will represent the retarding power, and its equivalent, that portion of the projectile force expended in overcoming it: hence, these various powers being exactly balanced, there is no tendency in the machine but to proceed in its path, with its remaining portion of projectile force.

Principle of stability in directing the path of the machine.

The stability in this position, arising from the centre of gravity

Remarkable

* A very simple experiment will show the truth of this theory. Take a circular piece of writing paper, and folding up a small portion, in the line of two radii, it will be formed into an obtuse cone. Place a small weight in the apex, and letting it fall from any height, it will steadily preserve that position to the ground. Invert it, and, if the weight be fixed, like the life boat, it rights itself instantly.

circumstance aiding this stability.

gravity being below the point of suspension, is aided by a remarkable circumstance, that experiment alone could point out. In very acute angles with the current it appears, that the centre of resistance in the sail does not coincide with the centre of its surface, but is considerably in front of it. As the obliquity of the current decreases, these centres approach, and coincide when the current becomes perpendicular to the sail. Hence any heel of the machine backward or forward removes the centre of support behind or before the point of suspension; and operates to restore the original position, by a power, equal to the whole weight of the machine, acting upon a lever equal in length to the distance the centre has removed.

A rudder necessary similar to the tail of a bird.

To render the machine perfectly steady, and likewise to enable it to ascend and descend in its path, it becomes necessary to add a rudder in a similar position to the tail in birds. Let F G be the section of such a surface, parallel to the current; and let it be capable of moving up and down upon G, as a centre, and of being fixed in any position. The powers of the machine being previously balanced, if the least pressure be exerted by the current, either upon the upper or under surface of the rudder, according to the will of the aeronaut, it will cause the machine to rise or fall in its path, so long as the projectile or propelling force is continued with sufficient energy. From a variety of experiments upon this subject I find, that, when the machine is going forward with a superabundant velocity, or that which would induce it to rise in its path, a very steady horizontal course is effected by a considerable depression of the rudder, which has the advantage of making use of this portion of sail in aiding the support of the weight. When the velocity is becoming less, as in the act of alighting, then the rudder must gradually recede from this position, and even become elevated, for the purpose of preventing the machine from sinking too much in front, owing to the combined effect of the want of projectile force sufficient to sustain the centre of gravity in its usual position, and of the centre of support approaching the centre of the sail.

Further use of the rudder.

The elevation and depression of the machine are not the only purposes, for which the rudder is designed. This appendage

pendage must be furnished with a vertical sail, and be capable of turning from side to side, in addition to its other movements, which effects the complete steerage of the vessel.

Experiments have been made on a large scale.

All these principles, upon which the support, steadiness, elevation, depression, and steerage, of vessels for aerial navigation, depend, have been abundantly verified by experiments both upon a small and a large scale. Last year I made a machine, having a surface of 300 square feet, which was accidentally broken before there was an opportunity of trying the effect of the propelling apparatus; but its steerage and steadiness were perfectly proved, and it would sail obliquely downward in any direction, according to the set of the rudder. Even in this state, when any person ran forward in it, with his full speed, taking advantage of a gentle breeze in front, it would bear upward so strongly as scarcely to allow him to touch the ground; and would frequently lift him up, and convey him several yards together.

The best mode of producing the propelling power only remains.

The best mode of producing the propelling power is the only thing, that remains yet untried toward the completion of the invention. I am preparing to resume my experiments upon this subject, and state the following observations, in the hope that others may be induced to give their attention towards expediting the attainment of this art.

Flight of birds.

The act of flying is continually exhibited to our view; and the principles upon which it is effected are the same as those before stated. If an attentive observer examines the waft of a wing, he will perceive, that about one third part, toward the extreme point, is turned obliquely backward; this being the only portion, that has velocity enough to overtake the current, passing so rapidly beneath it, when in this unfavourable position. Hence this is the only portion that gives any propelling force.

To make this more intelligible, let A B, Fig. 4, be a section of this part of the wing. Let C D represent the velocity of the bird's path, or the current, and E D that of the wing in its waft: then C E will represent the magnitude and direction of the compound or actual current striking the under surface of the wing. Suppose E F, perpendicular

dicular to A B, to represent the whole pressure; E G being parallel to the horizon, will represent the propelling force; and G F, perpendicular to it, the supporting power. A bird is supported as effectually during the return as during the beat of its wing; this is chiefly effected by receiving the resistance of the current under that portion of the wing next the body where its receding motion is so slow as to be of scarcely any effect. The extreme portion of the wing, owing to its velocity, receives a pressure downward and obliquely forward, which forms a part of the propelling force; and at the same time, by forcing the hinder part of the middle portion of the wing downward, so increases its angle with the current, as to enable it still to receive nearly its usual pressure from beneath.

Flight of the common rook.

As the common rook has its surface and weight in the ratio of a square foot to a pound, it may be considered as a standard for calculations of this sort; and I shall therefore state, from the average of many careful observations, the movements of that bird. Its velocity, represented by C D, Fig. 4, is 34·5 feet per second. It moves its wing up and down once in flying over a space of 12·9 feet. Hence, as the centre of resistance of the extreme portion of the wing moves over a space of 0·75 of a foot each beat or return, its velocity is about 4 feet per second, represented by the line E D. As the wing certainly overtakes the current, it must be inclined from it in an angle something less than 7° for at this angle it would scarcely be able to keep parallel with it, unless the waft downward were performed with more velocity than the return; which may be and probably is the case, though these movements appear to be of equal duration. The propelling power, represented by E G, under these circumstances, cannot be equal to an eighth part of the supporting power G F, exerted upon this portion of the wing; yet this, together with the aid from the return of the wing, has to overcome all the retarding power of the surface, and the direct resistance occasioned by the bulk of the body.

Very acute angles differ but little in their resistance.

It has been before suggested, and I believe upon good grounds, that very acute angles vary little in the degree of resistance they make under a similar velocity of current. Hence

Hence it is probable, that this propelling part of the wing receives little more than its common proportion of resistance, during the waft downward. If it be taken at one third of the whole surface, and one eighth of this be allowed as the propelling power, it will only amount to one twenty-fourth of the weight of the bird; and even this is exerted only half the duration of the flight. The power gained in the return of the wing must be added, to render this statement correct, and it is difficult to estimate this; yet the following statement proves, that a greater degree of propelling force is obtained, upon the whole, than the foregoing observations will justify. Suppose the largest circle that can be described in the breast of a crow, to be 12 inches in area. Such a surface, moving at the velocity of 34·5 feet per second, would meet a resistance of 0·216 of a pound, which, reduced by the proportion of the resistance of a sphere to its great circle (given by Mr. Robins, as 1 to 2·27) leaves a resistance of 0·095 of a pound, had the breast been hemispherical. It is probable however, that the curve made use of by Nature to avoid resistance, being so exquisitely adapted to its purpose, will reduce this quantity to one half less than the resistance of the sphere, which would ultimately leave 0·0475 of a pound as somewhat approaching the true resistance. Unless therefore the return of the wing gives a greater degree of propelling force than the beat, which is improbable, no such resistance of the body could be sustained. Hence, though the eye cannot perceive any distinction between the velocities of the beat and return of the wing, it probably exists, and experiment alone can determine the proper ratios between them.

From these observations we may, however, be justified in the remark—that the act of flying, when properly adjusted by the Supreme Author of every power, requires less exertions than, from the appearance, is supposed.

The act of flying requires less exertion than commonly supposed.

Brompton, Nov. 6th, 1809.

(To be continued in our next.)

II.

II.

Farther Remarks on Respiration, in Answer to J. F. *By Mr.* Acton.

To Mr. NICHOLSON.

My dear Sir,

Mr. Acton's motives for his experiments on germination and respiration

AS I am not conscious of having been influenced by any worse motive than the love of inquiry and truth in the prosecution of the experiments on germination and respiration, which you have done me the honour of inserting in your valuable Journal, I the more readily notice the ramarks of your correspondent J. F. in the last number*, being persuaded they were made from the purest intentions of doing justice to the talents and exertions of a very respectable individual, and correcting what may have appeared to him to have the semblance of misrepresentation.

In answer to Mr. Ellis.

I entreat it may be understood, that in speaking of Mr. Ellis's work nothing could have been farther from my thoughts, than any wish to lessen its merit, or detract from its value in the public estimation. Mr. Ellis thought proper to attempt the establishment of a new fundamental doctrine in chemical physiology; and for my own part being warmly attached to the more plain and simple manner of explaining the same phenomena, I presumed to lift up my feeble voice against the innovation, with what succefs cannot at present perhaps be determined.

Points in question.

Upon referring to Mr. Ellis's work and my papers, I believe it will be found, that the question at issue is precisely this. Do seeds, plants, and animals, during their respective functions of germination, vegetation, and respiration, absorb oxigen gas and emit carbonic acid gas? Or is solid carbon given out, and by uniting to the oxigen gas exteriorly to the seed, plant, or animal, does it form the carbonic acid gas produced? Mr. Ellis is of the latter opinion, and his work is written expressly to demonstrate it.

Opinion of Mr. Ellis.

* Vol. XXIV. p. 307.

He

He quotes a great number of experiments, and institutes a few of his own in support of it. Among those quoted is that of Bichat alluded to by J. F. Now the general tendency of Mr. Ellis's work, as far as it concerns respiration, is to prove the impossibility of oxigen gas being absorbed by the blood in the lungs during respiration. In my paper I observed, it was wonderful that Mr. Ellis should adduce the experiments of Bichat in support of this proposition, when Bichat stated it as affording a " proof of the passage of air into the blood through the lungs, in addition to that of healthy respiration." And I now add, is it not also *very unaccountable*, how Mr. Ellis should have overlooked those circumstances, which must destroy all analogy between the absorption of air in the lungs, and the injection of air into the vessels?

Bichat's experiment to prove the passage of air into the blood.

I very much hope neither your correspondent J. F., nor any of your readers, will suspect me of entertaining so preposterous a notion as that of the oxigen gas entering the blood vessels and continuing there in an aeriform state. On the contrary I have expressly declared, that death must be the consequence of such an event, no doubt by its mechanical action in impeding the circulation of the blood. I contend, the oxigen gas is absorbed chemically by the blood, which thus becomes changed from venous to arterial: the air consequently loses its elasticity and mechanical agency, as much as when it is combined with a metal or base to form an oxide or acid.

But the oxigen does not remain as a gas, it is chemically combined with the blood.

Mr. Ellis appears to have rested his principal argument against the absorption of oxigen gas by the blood upon what he conceives to be the impossibility of any chemical attraction taking place between them, on account of the interposed membrane, which separates the air in the lungs from the blood in the vessels.

Mr. Ellis supposes, the intervening membrane prevents this union.

To prove the contrary of this, suffer any small animal to die in oxigen gas, and immediately lay bare the thoracic viscera; observe them carefully for some time, and you will perceive a gradual change take place in the colour of the blood, in the lungs particularly. Compare this with another just opened, and the difference will be more obvious. Also examine the brain of an animal recently dead, the veins of the

The contrary proved

the pia mater will appear quite purple; but after a little time they will begin to assume a degree of floridity, which can be accounted for in no other way than by the absorption of oxigen gas. Here then it seems the membranes are no positive impediment to its absorption. And if this so readily happen in dead animals, with how much more facility must it occur in those possessing vitality?

By the absorption of oxigen when a membrane intervenes.

If an animal be killed by injecting air into the jugular or any other vein, it is not fair to infer the nonabsorption of oxigen gas by the blood through the lungs; in the former instance the point of contact is very trifling, and the absorption of course limited; neither is there any mode by which the carbonic acid gas can be emitted. The healthy functions of the vessels therefore must be destroyed, and the animal die. In the lungs it is very different. The human lungs expose an immense surface, not less than 21907 square inches, or ten times more than the whole body; and it is obvious, that the chemical action between the air and the blood must be considerably influenced by the surface of contact.

Death from injecting air into a vein no proof, that it cannot be absorbed by the blood in the lungs.

It is not so difficult to prove the absorption of oxigen gas by the blood, as to account satisfactorily for the formation of the carbonic acid gas emitted. That it ought to be considered excrementitious, and as the effect of the absorption of the oxigen gas, is my firm belief. Nature is so wise in her laws, that she will not permit continued repletion without some adequate evacuation. The oxigen gas no doubt acts as a nutrient and stimulant to the blood; the carbonic acid gas being the superfluous matter carried off. And surely it must be much more easily extricated in an aeriform than a solid state. My experiments on germination evinced with how much facility this gas is given out by seeds in a variety of instances, even when oxigen gas is not present; as also in spontaneous and putrid decomposition. Hence it appears, there need not be so much difficulty thrown in the way of rationally accounting for it, when produced by the healthy functions of plants and animals; for in the former case at any rate no reference can be made to the issuing of the solid carbon, and its uniting to the oxigen; nor can I see any sufficient reason, why the carbonic acid may

Difficulty to account for the formation of carbonic acid.

Carbonic acid gas given out by seeds, when no oxigen gas is present.

may not be found interiorly in the animal or plant, as well in one case as in the other.

I am certain it was not my wish to quote Mr. Ellis's work unfairly, and I do not conceive I have done so. However as your correspondent J. F. thinks I should have undertaken to show rather that Mr. Ellis contradicted himself, than that he perverted the experiments of Bichat, for his satisfaction I must inform him I could easily have done so, if it be admitted, that two experiments of an opposite nature, mentioned doubtless to prove the truth of the general proposition, can be considered a contradiction. In page 118 of his work is the following passage. "Nor when air "was forced down the windpipe of a dog in the experi-"ments of Dr. Hales, was it able to pass into the pulmo-"nary artery or veins:" and at page 128, "By forcing air "through the windpipe into the lungs with a syringe, and "confining it there, he (Mr. Bichat) has made it to enter "into the blood vessels."

Mr Ellis adduced two contradictory experiments.

I cannot take leave of your correspondent without remarking upon the unfairness of his statement in his postscript. I appeal to yourself and readers if he have stated the question at issue with accuracy: and as to his leaning to the side of Mr. Ellis on account of the experiments of Messrs. Allen and Pepys and others, I can only say, the strong impression on my mind at the time I read them was directly the reverse. In the account of the experiments of Messrs. Allen and Pepys, at page 203 of the 22d vol. of your Journal, is the following passage. "In this recital of experiments, which "have occupied a considerable portion of time and atten-"tion, we have endeavoured to give a plain statement of "facts, from which every one may draw conclusions for "himself." Clearly showing their own minds were by no means made up to decide upon the truth of any particular theory. And in the following page it runs thus: "When "respiration is attended with distressing circumstances, as "in the 14 and 15 experiments, *there is reason to conclude,* "*that a portion of oxigen is absorbed*; and in the last of "these experiments we may remark, that, as the oxigen "decreases in quantity, perception gradually ceases, and we "may

Experiments of Messrs. Allen & Pepys

"may suppose, that life would be completely extinguished "on the total abstraction of oxigen."

in favour of the absorption of oxigen by the blood.

Surely this cannot be considered as supporting the opinion of the impossibility of oxigen gas being absorbed by the blood.

My time has been so entirely taken up by other indispensable avocations, I have not yet had an opportunity of completing my experiments on vegetation. As soon as they can be finished, I shall do myself the pleasure of transmitting them.

I remain, dear Sir,

Yours' &c.

J. ACTON.

III.

On the Dionæa Muscipula. Read at the Literary and Philosophical Society of Manchester, Oct. 6th, by Mr. Robert Lyall. *Communicated by the Author.*

Leaves of Venus's fly-trap contract when irritated.

THE dionæa muscipula has no less astonished the eye of the philosopher or of the physiologist, than that of the common observer, by the singular contraction of its leaves when irritated by a stimulus. Most authors have ascribed the motion of the leaves of this plant to an irritable principle; but Sennebier and a few others have endeavoured to show, that it is owing to a mechanical cause.

Their mechanism.

At the apex of each lanceolate leaf of this plant there is a pair of ovate toothed lobes, which when irritated approach each other, at the same time that the spines of both lobes cross, "like the teeth of a spring rat-trap."

Broussonet ascribes the contraction to the escape of a fluid.

Sennebier, who is always unwilling to admi tthat vegetables are irritable, mentions the following opinion of Broussonet concerning the cause of this motion. "Broussonet, in the Memoirs of the Academy of Sciences of Paris for 1748, suspects, that the disengagement of some fluids influences these motions; the little glands, which we discover on the leaves of the

the dionæa muscipula, are scarcely pricked by an insect, when the leaves fold upon themselves, and seize the insect. The pricking occasions the discharge of a fluid, which holds the leaves open by filling their vessels. Accordingly when the plantis young, when the little glandulous bodies are not formed, when they are empty, or when the fluids do not flow abundantly, the leaves are folded upon themselves just in the same manner as when they were pricked."

However ingenious this theory may appear to others, it does not seem to be juſt. I havealready noticed Broussonet's opinion with regard to the motions of the leaves of the indigenous species of drosera, and think I have shown it to be unsupported by fact. In the present instance a very simple experiment will be sufficient in my opinion, to allow us to conclude, that the motions of the leafy appendages* of the dionæa must be owing to some other cause, than that juſt noticed. If we slightly touch the inside of one of these armed lobes with a straw, or other soft body, so as not to injure its structure in the least; the contraction almost instantaneously commences, nor does it cease, till the lobes either come into contact with the straw, or other offending body, or with each other, if the offending body be removed. Now in this experiment the structure of the leaf is not injured, no fluid escapes, but yet the contraction is complete; hence it must be obvious, that the discharge of a fluid, which could hold the leaves open by filling their vessels, cannot be the cause of the motion.

This theory not just.

They contract when no fluid can escape.

Nearly at the end of the chapter on irritability Sennebier mentions some ideas, which Camparetti entertained. He explains the motions of the sensitives by the air vessels (trachées) of the petiole of the leaf &c.; and supposes, that they are filled with a very elastic *aqueo-aerial* fluid. Sennebier says, that Camparetti believed he could explain in the same manner the motions of the dionæa, drasera, &c. This hypothesis seems to me so improbable, so contrary to analogy, and so inadequate to explain the phenomenon (allowing the vessels did exist filled with this aqueo-aerial fluid,

Camparetti's theory.

* The lobes of the dionæa, which contract, are properly a leafy appendage (Smith's introduction) though sometimes I have called them the leaf

which

which of course would be governed by light, heat, or moisture); that I shall not endeavour to refute it.

The contraction is owing to a stimulus.

We have now seen, that the contraction of the leaf of the dionæa muscipula takes place in consequence of the application of a stimulus; and that it does not seem probable, that this action can be explained on mechanical principles. What inference then must we draw? In my opinion we must again be forced to infer the same conclusion as we did with respect to the species of drosera, namely, that this plant is possessed of an inherent power, by which, when stimulated, it is enabled to contract; and if this inference be just, we would say, that the leaves possess irritability.

In considering the motions of this plant it must always be remembered, that it seems to move purely in consequence of the application of a stimulus.

Sensitive plant. Contraction of its leaves.

I shall next proceed to the mimosa pudica. It is well known, if we take a leaf of this plant similar to what is represented Pl. III, fig. 5, and then by means of a pair of scissars (completely dry), cut off half the pinnula A, this pinnula will contract at its joint either immediately, or in a few seconds; its neighbour, or opposite pinnula, B, closing at the same time, or soon after. The pinnulæ A and B having come into contact, there will be a pause or a short cessation of motion, but in the course of a few more seconds the next pair of pinnulæ, CC, will also shut up, and the same will happen with every pair of pinnulæ of that pinna successively; only with this difference, that the intervals between the shutting up of each pair of pinnulæ will be shorter, the farther they are from the pinnula that was cut. After the whole of the pinnulæ of this pinna have completely closed, and a little interval; then the joint D will become affected i. e. it will bend so as to allow the pinna to drop considerably. Nevertheless, the motion is often not so obvious in this joint as in that to be mentioned. A longer pause will now intervene, in some cases so long as to make us suppose that all motion is at an end; but at length, the joint E suddenly bends, and astonishes the beholder. The petiole F now instead of forming an acute angle with the stem above the joint, forms a very obtuse angle with it. We shall now have another cessation of motion, and then the

the joint II will slightly bend; then another pause; then a shutting up of the pair of pinnulæ, II, and so on with the other pinnulæ, till the whole pinna is closed. The motions however will not be so regular in this pinna, as they were in the other, for as the pinnulæ II approach they press forwards the next pair, and so on with all the rest. If we take another leaf similar to that of which I have given a sketch, and cut the pinnula of the pinna A, B, C, or D, fig. 6, in the same manner, a variety of beautiful experiments may be made; which will strike the experimenter with amazement, and excite an anxiety in his mind to ascertain the cause of this motion.

The motion not to be explained on mechanical principles.

The above related experiment I think sufficient for my present purpose; which is to endeavour to show, that these motions cannot be explained on any mechanical principles, and that they very probably depend on the irritability of the plant.

It does not appear, that the motion is occasioned by impulse: for a bit of the pinnula may be cut off almost without producing any motion. But allowing that a little motion were produced in the injured pinnula, it comes naturally, as a question why does the motion become so extensive? how is the impulse communicated to the origin of the petiole? These questions I believe will never be satisfactorily answered upon the principle mentioned.

Examination of Mrs Ibbetson's theory.

Camparetti's theory will be liable to nearly the same objections as that of Mrs. Ibbetson noticed below. I do not here intend to deny, that a structure similar to what Mrs. Ibbetson describes is to be found in the mimosa sensitiva; although I confess my doubts have increased of late. I am willing to allow, that all is accurate, and shall proceed now to inquire, whether by such a structure, acted upon by heat, light, or moisture, we could possibly explain the motions of the mimosa pudica. In the experiments related above, I presume no one would say, that moisture was the cause of the motion; as the scissars were quite dry. It is to be remembered also, that this plant will perform its motions under water. As there was no change of light, consequently this had no share in the effect. Besides, when moisture is produced in consequence of the abstraction of light, all the pinnulæ shut up at the same time: not in the regular

The motion not produced by moisture, light, darkness,

or change of temperature.

regular order mentioned in the experiment. Neither did the motion take place from change of temperature, for the temperature was not altered.

How then?

A great many questions will here suggest themselves, as, How does it happen, that the motion is produced? How does it become so extensive? How comes it that there are such regular motions and p·-ɹses? &c. I expect at some future period to be able to throw some light on this difficult subject, but in the mean time must beg leave again to quote Dr. Smith's words, and say, that " it is vain to attempt any mechanical solution of the phenomenon" above related, which would seem to depend on an " exquisite irritability" in the plant itself.

Sundew not affected by light, heat, moisture, or their opposites.

P. S. I might have added at the end of my last, that the leaves of the drosera rotundifolia and longifolia remain completely expanded during the hottest sunshine and driest weather; during the coldest and wettest weather*; during the greatest darkness, and finally during the brightest light of day. Here then, neither heat, cold, dryness, dampness, darkness, nor light in general at all effect the leaves; but if a foreign body is applied to the leaf so as to stimulate, then it will shut up in the manner related in my last. How will this agree with Mrs. Ibbetson's ideas?

IV.

Questions on the Study of the Mosses. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

How should the mosses be studied?

I Should consider myself obliged, if you should think the following questions of sufficient import to merit a place in your Journal. In commencing the study of the musci, should

* This is to be taken in a limited sense, i. e. only during the expansion of the leaves, not during the cold of winter.

the

the generic divisions pointed out by Hedwig, and by Dr. Smith be followed? Or are these divisions too difficult for a beginner? Ought not distinctions similar to those of Hithering to be adopted, which are more obvious to a beginner? Is not a good introduction to the cryptogania class still wanted? How would such an introduction be received? and How should it be conducted?

T. L. N.

V.

Analysis of the compact red Iron Ore in cubic Crystals from Toeschnitz in Thuringia. By Mr. Bucholz*.

Being at Ilmenau in june 1786, I had the pleasure of seeing my old friend, mine-counsellor Voigt, who showed me a mineralogical rarity lately discovered, a compact red ore of iron crystallized in cubes. He presented me with a specimen in which the crystals were still on their gangue, in order that I might analyse them: but on my remarking, that the crystals when separated would be in too small quantity, and might retain a little of the gangue, he gave me some perfectly pure separate crystals, and communicated to me the particulars of the situation where they were found.

Red iron ore in cubic crystals lately discovered

This species of minerals is found at Toeschnitz in Thuringia. The gangue of the crystals is compact red iron ore passing into a primitive argillaceous schist. The cut in which they were found has been given up, because the iron ore was too poor; and it is to be apprehended, that, when this is once filled, mineralogifts will no longer be able to procure it.

in Thuringia.

Its gangue.

It would be superfluous here to give a description of the compact red iron ore, which is to be found in all treatises on mineralogy, and agrees with this mineral; except that it is in perfectly regular cubes, differing in size from that of a

Appearance.

* Journal des Mines, vol. XXII, p. 435.

lentil to that of a pea. The smallest crystals are frequently fixed on the largest.

Lost nothing by heat.

a. A hundred grains of this substance, exposed for half an hour to a very brisk red heat, suffered no loss.

Treated with muriatic acid

b. A hundred grains were powdered in an agate mortar. The powder had the same brown red colour of oxide of iron at a maximum heated redhot in the fire. It was boiled for a moment with concentrated muriatic acid of the spec. grav. of 1·165. No oximuriatic acid was formed during the solution. The whole was dissolved except a few small grains of quartz, which certainly were no constituent principles of the mineral; since they were not coloured by iron.

Few grains of quartz.

c. The solution *b*, which had the same colour as muriatic solutions of brown red oxide of iron, was divided into two equal portions, and one of these was subjected to the following trials.

No alumine,

α. Part of it was decomposed by caustic potash in excess, and the solution was boiled on the precipitate. After filtering and neutralizing with muriatic acid, caustic ammonia was added, which did not form the slightest cloud in the solution; but it would have become turbid, if it had contained any alumine or other substance dissolved by the caustic potash.

barytes, strontian,

β. Diluted sulphuric acid occasioned no precipitate in the first solution, whence it follows, that the precipitate contained neither barytes nor strontian.

lime,

γ. Another portion was decomposed by an excess of ammonia, and the precipitate separated by filtration. Oxalate of ammonia added to the colourless liquid did not render it in the least turbid. It contained no lime therefore.

magnesia,

δ. Another part of the first solution having been decomposed by ammonia, and the precipitate separated, the supernatant fluid was boiled with carbonate of soda; and as it underwent no change, it certainly contained no magnesia.

manganese or other substance.

ε. Muriate of barytes did not render the solution turbid.

d. The other half of the solution *b* was neutralized by ammonia, and the oxide of iron precipitated by succinate of

of ammonia: after which I sought in vain to detect manganese, lime, or any other substance, in the filtered liquor.

This mineral is consequently a pure oxide of iron crystallized in cubes; and according to my last researches it consists of 70·5 iron with 29·5 oxigen.

A pure oxide of iron.

VI.

Extract of a Letter from Mr. COUTELLE, *Subinspector at Reviews, to Mr.* GUYTON-MORVEAU, *on the Parabolic Lens of Rospini, purchased at Vienna for the French Government*.*

THIS lens is about a metre [3 feet 3 in.] in diameter, and $2\frac{1}{2}$ met. [8 f. 4 in.] focus. It is composed of two pieces of glass, united together by a hoop of iron, so as to form a hollow vessel capable of holding 80 or 90 quarts of spirit of wine.

Large burning lens,

It was made at Gratz, in Styria, by Rospini, a celebrated mechanist, for some alchemists. It was not cast, but softened by heat, and bent over a parabolic mould. Several pieces were broken before he succeeded, so that it cost originally 20 or 30 thousand francs [from 800 to 1200 guineas]. Mr. Coutelle paid for it 2900 florins [£ 338] in paper money.

made for some alchemists.

Mr. Jacquin of Vienna, and several men of science, who were witnesses to the experiments, say, that it burned a diamond in a few seconds, and fused platina in a few minutes. A button of platina weighing 29 grains was melted by it, and made in part to boil. The diameter of the focus does not appear to exceed four lines.

Its effects.

The lens, with the apparatus for placing the object of the experiment, fixed on a platform, and placed on an inclined plane mounted on a strong frame, is made to follow the course of the sun by means of machinery regulated by a pendulum beating seconds.

Made to move with the sun.

When made to incline toward the east or west it requires a counterpose, otherwise it would be liable to fall, being of the weight of 250 kilogr. [550 lbs avoird.].

* Abridged from the Annales de Chimie, vol. LXIX, p. 92.

 VII.

VII.

An Account of a Method of dividing Astronomical and other Instruments, &c. By Mr. Edward Troughton.

(Concluded from page 18.)

True divisions to be made from the erroneous dots.

HAVING now completed the two first sections of my method of dividing; namely, the first, which consists of making 256 small round dots; and the second, in finding the errours of these dots, and forming them into a table; I come now to the third and last part, which consists in using the erroneous dots in comparison with the tabulated errours, so as ultimately to make from them the true divisions.

Subdividing sector described.

It will here be necessary to complete the description of the remaining part of the apparatus. And first, a little instrument which I denominate a subdividing sector presents itself to notice. From all that has hitherto been said, it must have been supposed, that the roller itself will point out, upon the limb of the instrument to be divided, spaces corresponding to others previously divided upon itself, as was done in setting off the 256 points: but, to obviate the difficulty of dividing the roller with sufficient exactness, recourse was had to this sector; which also serves the equally important purpose of reducing the bisectional points to the usual division of the circle. This sector is represented of half its dimensions by Fig. 5, Pl. I. It is formed of thin brass, and centered upon the axis at A, in contact with the upper surface of the roller: it is capable of being moved round by hand; but, by its friction upon the axis and its pressure upon the roller, it is sufficiently prevented from being disturbed by accident. An internal frame B B, to which the arc C C is attached, moves freely in the outer one, and by a spring D is pushed outwards, while the screw E, the point of which touches the frame B, confines the arc to its proper radius. The arc of this sector is of about four times greater radius than the roller, and upon it are divided the spaces which must be transferred to the instrument, as represented on a magnified scale by Fig. 4. Now, the angle of one of the spaces of the circle will be measured

sured by sixteen times its angular value upon the sectorial arc, or 22° 30′; but this does not represent any number of equal parts upon the instrument, the subdivisions of which are to be 5′ each; for $\frac{1° 24′ 22″5}{5}$ is exactly 16⅞, therefore so many divisions are exactly equal to a mean space between the dots, the errours of which have been tabulated. Let, therefore, the arc of the sector be divided into 16 spaces of 1° 20′ each, and let a similar space at each end be subdivided into eight parts of 10′ each, as in Fig. 4; we shall then have a scale which furnishes the means for making the true divisions, and an immediate examination at every bisectional point.

Dividing by the engine.

I have always divided the sector from the engine, because that is the readiest method, and inferior to none in point of accuracy, where the radius is very short; but, as it is more liable than any other to centrical errour, the adjustment of the arc by the screw E becomes necessary: by that adjustment, also, any undue run in the action of the roller may be reduced to an insensible quantity *.

Division by lines preferable to dots.

When the utmost degree of accuracy is required, I give the preference to dividing by lines, because they are made with a less forcible effort than dots are; and also because, if any small defect in the contexture of the metal causes the cutter to deviate, it will, after passing the defective part, proceed again in its proper course, and a partial crookedness in the line will be the only consequence; whereas a dot, under similar circumstances, would be altogether displaced. But, on the other hand, where accuracy has been out of the question, and only neatness required, I have used dots; and I have done so, because I know that when a dot and the wire which is to bisect it are in due proportion to each other, (the wire covering about two thirds of the dot) the nicest comparison possible may be obtained. It may be farther observed, that division by lines is complete in itself; whereas that by dots requires lines to distinguish their value.

Apparatus for cutting the divisions.

On the upper side of Fig. 1 is represented the apparatus for cutting the divisions. It consists of three pieces J K L,

* See note page 130.

jointed

jointed together so as to give to the cutter an easy motion for drawing lines directly radiating from the centre, but inflexible with respect to lateral pressure; *d d* are its handles. The cutting point is hidden below the microscope H; it is of a conical form, and were it used as a dotting point, it would make a puncture of an elliptical shape, the longer diameter of which would point towards the centre. This beautiful contrivance, now well known, we owe to the ingenuity of the late Mr. Hindley of York; it was borrowed by Mr. Ramsden*, and applied with the best effect to his dividing engine.

Invented by Mr. Hindley.

It might have been mentioned sooner, that in the instance which I have selected as an example of my dividing, the operation took place when the season of the year, and the smoke of London, had reduced the day to scarcely six hours of effective light; and rather than confine my labours within such narrow limits, I determined to shut out the day-light altogether. Fig. 7 shows the construction of the lanterns which I used. A very small wick gave sufficient light, when kept from diverging by a convex lens; while the inclining nossel was directed down exactly upon the part looked at, and the light, having also passed through a thin slice of ivory, was divested of all glare. I enter into this description, because, I think, I never saw my work better, nor entirely to so much advantage as in this instance; owing, perhaps, to the surrounding darkness allowing the pupil of the eye to keep itself more expanded, than when indirect rays are suffered to enter it. The heat from a pair of these lanterns was very inconsiderable, and chiefly conducted along with the smoke up the reclining chimney.

Lanterns employed.

Previous to cutting the divisions, the parts now described must be adjusted. The cutting apparatus must be placed with the dividing point exactly at the place where the first line is intended to be drawn, and clamped, so that the adjusting screw may be able to run it through a whole interval. The microscope H must be firmly fixed by its two pillars *b b* to the main frame, with its micrometer head at *zero;* and with its only wire in the line of the radius, bi-

Preparation for cutting the divisions.

* This I learned from that most accurate artist Mr. John Stancliffe, who was himself apprentice to Hindley.

secting

secting the first of the 256 dots. And it should be observed, that the cutting frame and this must not vary respecting each other, during the time that the divisions are cut; for any motion that took place in either would go undiminished to the account of errour. The microscope I is also fastened to the main frame; but it is only required to keep its position unvaried, while the divisions of the sector pass once under its notice; for it must have its wires adjusted afresh to these divisions at every distinct course. The microscope I has two wires, crossing each other at an angle of about 40°; and these are to be placed so as to make equal angles with the divisions of the sector, which are not dots, but lines. The sectorial arc must also be adjusted to its proper radius by the screw E, Fig. 5; *i. e.* while the main frame has been carried along the circle through a mean interval shown by H, the sector must have moved through exactly 16⅞ of its divisions, as indicated by I *.

Cutting the divisions.

Things being in this position; after having given the parts time to settle, and having also sufficiently proved the permanence of the micrometer H and the cutting frame with respect to each other, the first division may be made; then, by means of the screw for slow motion, carry the apparatus forward, until the next line upon the sector comes to the cross wires of I; you then cut another division, and thus proceed until the 16th division is cut, = 1° 20'. Now the apparatus wants to be carried farther, to the amount of ⅞ of a division, before an interval is complete; but at this last point no division is to be made; we are here only to compare the division on the sector with the corresponding dot upon the instrument. This interval, however, upon the

* For the sake of simplicity, the account of the process is carried on as if the roller measured the mean interval without errour: But it was said (Page 8) that the roller, in a continued motion quite round the circle, would in some part of its course err by 30" or more; therefore, when this is the case, an extreme run of the roller cannot agree with a mean interval of the circle nearer than $\frac{30''}{128} = 0.23''$; and most probably this kind of errour will on some intervals amount to double that quantity. It therefore becomes matter of prudent precaution, to examine every interval previous to making the divisions; and where necessary, to adjust the sector, so that its arc may exactly measure the corresponding interval as corrected by the tabulated errours.

circle

circle will not be exactly measured by the corresponding line of the sector, which has been adjusted to the mean interval, for the situation of the dot 1°·4 is too far back, as appears by the table of real errours, by — 4·8 divisions of the micrometer head. The range of the screw for slow motion must now be restored, the cross wires of H set back to — 4·8 divisions, and the sector moved back by hand, but not to the division 0, where it began before; for, as it left off in the first interval at $\frac{7}{8}$ of a division, it has to go forwards $\frac{1}{8}$ more before it will arrive at the spot where the 17th division of the instrument 1° 25′ is to be made, so that in this second course it must begin at $\frac{1}{8}$ short of 0. Go through this interval as before, making a division upon the circle at every one of the 16 great divisions of the sector; and H should now reach the third dot, allowing for a tabular errour of — 10·2 when the division $\frac{6}{8}$ths of the sector reaches the cross wires of I. It would be tedious to lead the reader through all the variety of the sector, which consists of eight courses; and it may be sufficient to observe, that at the commencement of every course, it must be put back to the same fraction of a division which terminated its former one; and that the wire of the micrometer H must always be set to the tabular errour belonging to every dot, when we end one interval and begin another. The eight courses of the sector will have carried us through $\frac{1}{32}$ part of the circle, 11° 15′, and during this time, the roller will have proceeded through half a revolution; for its close contact with the limb of the circle does not allow it to return with the sector, when the latter is set back at every course. Having in this manner proceeded, from one interval to another, through the whole circle, the micrometer at last will be found with its wire, at *zero*, on the dot from which it set out; and the sector, with its 16th division, coinciding with the wires of its microscope.

Advantages of this method

Having now given a faithful detail of every part of the process of dividing this circle, I wish to remind the reader that, by verification and correction at every interval, any erroneous action of the roller is prevented from extending its influence to any distant interval. It will be farther observed, that the subdividing sector magnifies the work; that

that by means of its adjustable arc, it makes the run of the roller measure its corresponding intervals upon the circle; and, without foreign aid, furnishes the means of reducing the bisectional intervals to the usual division of the circle. Farthermore, the motion of the wire of the micrometer H, according to the division of its head and corresponding table of errours, furnishes the means of prosecuting the work with nearly the same certainty of success, as could have happened, had the 256 points been (which in practice is quite impossible) in their true places.

Why termed dividing by the eye.

Now the whole of my method of dividing being performed by taking short measures with instruments which cannot themselves err in any sensible degree, and, inasmuch as those measures are taken, not by the hand, but by vision, and the whole performed by only looking at the work, the eye must be charged with all the errours that are committed, until we come to cut the divisions; and, as in this last operation the hand has no more to do than to guide an apparatus so perfect in itself, that it cannot be easily made to deviate from its proper course, I would wish to distinguish it from the other methods by denominating it, DIVIDING BY THE EYE*.

The

Degree of accuracy of the sight & touch.

* I must here remark, that Smeaton has represented the greatest degree of accuracy that can be derived from vision, in judging of the coincidence of two lines, at $\frac{1}{4000}$ part of an inch. From this it may fairly be in[f]erred, tha[t h]e had not cultivated the power of the sight, as he had done that of the touch[, the] latter of which, with that ability which appeared in all his works, he rendered sensible to the $\frac{1}{60000}$ part of an inch. Were materials infinitely hard, no bounds could be set to the precision of contact; but taking things as they are, the different degrees of hardness in matter, may be considered as a kind of magnifying power to the touch, which may not unaptly be compared with the assistance which the eye receives from glasses. It is now quite common to divide the seaman's sextant to 10″, and a good eye will estimate the half of it; which, on an eight inch radius is scarcely $\frac{1}{10000}$ of an inch This quantity, small as it is, is rendered visible by a glass of one inch focal length; and such is the certainty with which these quantities are seen, that a seaman will sometimes complain that two pair of these lines will coincide at the same time; and this may happen, and yet no division of his instrument err by more than $\frac{1}{50000}$ part of an inch. All this is applicable to judging of the coincidence of *lines* with each other, and furnishes not the most favourable

Few persons capable of dividing originally.

The number of persons at all capable of dividing originally have hitherto been very few; the practice of it being so limited, that, in less than twice seven years, a man could hardly hope to become a workman in this most difficult art. How far I shall be considered as having surmounted these difficulties, I know not; but if, by the method here revealed, I have not rendered original dividing almost equally easy with what copying was before, I have spent much labour, time, and thought, in vain. I have no doubt indeed, that any careful workman who can divide in common, and has the ability to construct an astronomical instrument, will, by following the steps here marked out, be able to divide it, the first time he tries, better than the most experienced workman, by any former method.

Subdivision with the screw instead of the roller.

If, instead of subdividing with the roller, the same thing be performed with the screw, it will not give to dividing by the eye any very distinctive character. I have practised this on arcs of circles with success, the edge being slightly racked, the screw carrying forward an index with the requisite apparatus, and having a divided micrometer head; the latter answers to the subdividing sector, and, being used with a corresponding table of errours, forms the means of correcting the primitive points; but the roller furnishes a more delicate action, and is by far more satisfactory and expeditious.

Six feet circle for the Royal Observatory, dividing on its edge.

It is known to many, that the six feet circle, which I am now at work upon for our Royal Observatory, is to be divided upon a broad edge, or upon a surface at right angles to the usual plane of division: The only alterations, which will on this account be required, are, that the roller must act upon that plane which is usually divided upon; which roller,

vourable display of the accuracy of vision. But with the microscopes here described, where the wire bisects the image of a dot, or a cross wire is made to intersect the image of a line, by an eye practised in such matter, a coincidence may undoubtedly be ascertained to $\frac{1}{50000}$ part of an inch. I am of opinion, that as small a quantity may be rendered visible to the eye, as can by contact be made sensible to the touch; but whether Mr Smeaton's $\frac{1}{60000}$ and my $\frac{1}{50000}$ be not the same thing, I will not determine; the difference between them, however, is what he would no more have pretended to feel, than I dare pretend to see.

roller, being elevated or depressed, may be adjusted to the commensurate radius without being made conical, as was necessary in the other case. The apparatus, similar to the other, must here be fixed immovably to the frame which supports the circle: its position must be at the vertex, where also I must have my station; and the instrument itself must be turned around its axis, in its proper vertical position, as the work proceeds. The above may suffice, for the present, to gratify those who feel themselves interested upon a subject, which will be better understood, if I should hereafter have the honour of laying before the Royal Society a particular description of the instrument here alluded to; a task which I mean to undertake, when after being fixed in the place designed for it, which I hope will be effected at no very distant period, it shall be found completely to answer the purposes intended.

A circle may readily be divided centesimally,

Should it be required to divide a circle according to the centesimal division of the quadrent, as now recommended and used in France, we shall have no difficulty. The 100° of the quadrent may be conveniently subdivided into 10 each, making 4000 divisions in the whole round. The 256 bisectional intervals, the two tables of errours, and the manner of proceeding and acting upon them will be exactly the same as before, until we come to cut the divisions; and for this purpose we must have another line divided upon the sector. For $\frac{1}{4000}$ part of the circle being equal to $5'{\cdot}4$ of the usual angular measure $\frac{1^\circ\,24'\,22''{\cdot}5}{5{\cdot}4} = 15\frac{5}{8}$ divisions; and just so many will be equivalent to one of the intervals of the circle. The value of one of the great divisions of the sector will be $1^\circ\,26'\,24''$, and that of the $\frac{1}{8}$ parts, which are to be annexed to the right and left as before, will be $10'\,48''$, therefore divisible by the engine. Should any astronomer choose to have both graduations upon his instrument, the additional cost would be a mere trifle, provided both were done at the same time.

and in the common way at the same time.

Straight lines may be divided on the same principle.

It must already have been anticipated, that dividing by the eye is equally applicable to strait lines as it is to circles. An apparatus for this purpose should consist of a bar of brass,

brass, three quarters of an inch thick, and not less than three inches broad; six feet may do very well for the length; it may be laid upon a deal plank strengthed by another plank screwed edgewise on its lower surface. The bar should be planed, on both its edges and on its surface, with the greatest exactness; and it will be better, if it has a narrow slip of silver, inlaid through its whole length, for receiving the dots. An apparatus nearly similar to the other should slide along its surface, carrying a roller, the circumference of which is 12·8 inches, and turned a little conical for the sake of adjustment. The roller may be divided into 32 parts, each of which when transferred to the bar will give intervals of 0·4 of an inch each: The angle of the subdividing sector should of course be 11° 15, and subdivided into four parts, which will divide the inch into tenths: The surface may also receive other lines, with subdivisions suited to the different purposes for which it may be wante d. The revolutions of the roller and its $\frac{1}{32}$ parts must be dotted upon the bar; taking care, by sizing the roller, to come as near the true standard measure as possible: When this is done, compare the extent of the greater disectional number that is contained in the length; *i. e.* 128 intervals or 51·2 inches, with the standard measure; noting the difference as indicated by the micrometer heads. The examination and construction of the table of errours may then be conducted just as was done for the circle.

Aparatus for the purpose.

Method of applying it.

Being now ready for the performance of its work, the scale to be divided must be laid alongside of the bar, and the true divisions must be cut upon it by an appeal, as before, to the erroneous dots on the bar, corrected by a corresponding table of errours. The apparatus, remaining entire in the possession of the workman, with its primitive dots, the table of errours, &c., is ready for dividing another standard, which will be precisely similar to others that have been, or may be, divided from it. It may be considered, indeed, as a kind of engine; and as it is not vitiated by the coarse operation of racking with a screw, but performed by only looking at the work, the method will command about three times the accuracy that can be derived from the usual straight-line dividing engine. Should it be asked, if

an

an engine thus appointed would succeed for dividing circles? I answer, Yes; but I would not recommend it; because, beyond a certain extent of radius, it is not necessary; for the errours, which would be introduced into the work by the violence of racking a large wheel, are sufficiently reduced by the comparative shortness of the radius of such instruments as we divide by that method: And, what is still more to the purpose, the dividing engine is four times more expeditious, and bears rough usage better. I cannot quit the subject of dividing straight lines without observing, that I never had my apparatus complete. The standard which I made for Sir George Shuckburg Evelyn in 1796 was done by a mere make-shift contrivance, upon the principle of dividing by the eye; how I succeeded may be seen in Sir George's papers on Weights and Measures (Phil Trans. for 1798). I made a second, some years after, for Professor Pictet of Geneva, which became the subject of comparison with the new measure of France, before the National Institute; and their report, drawn up by Mr. Pictet, has been ably restated and corrected by Dr. Young, as published in the Journals of the Royal Institution. I made a third for the Magistrates of Aberdeen. I notice the two latter, principally to give myself an opportunity of saying, that, if those three scales were to be compared together, notwithstanding they were divided at distant periods of time, and at different seasons of the year, they would be found to agree with each other, as nearly as the different parts of the same scale agree.

Such an engine might be used for dividing circles, but not to be recommended.

Standard measures.

I hope I may here be allowed to allude to an inadvertency, which has been committed in the paper mentioned above; and which Sir George intended to have corrected, had he lived to conclude his useful endeavours to harmonize the discordant weights and measures of this country. The instruments which he has brought into comparison are, his own five feet standard measure and equatorial; General Roy's forty-two inch scale; the standard of Mr. Aubert; and that of the Royal Society. The inadvertency is this: In his equatorial, and the standard of the Royal Society, he has charged the errour of the most erroneous extent, when compared with the mean extent, alike to both divi-

Inadvertency of Sir G. Shuckburg Evelyn:

sions;

sions; *i. e.* he has supposed one of the divisions which bound the erroneous extent, to be too much to the right, and the other too much to the left, and that by equal quantities: This is certainly a good-natured way of stating the errours of work; and perhaps not unjustly so, where the worst part has been selected; but, in the other three instances, namely, in General Roy's, Mr. Aubert's, and his own standard, he has charged the whole errour of the most erroneous extent to one of the bounding lines.

General accuracy of Bird's dividing. The Greenwich quadrant

I was well confirmed in my high opinion of the general accuracy of Bird's dividing, when, last wanter*, I measured the chords of many arcs of the Greenwich quadrant. That instrument has indeed suffered both from a change in its figure, and from the wearing of its centre; but the graduation, considering the time when it was done, I found to be very good. Sir George in his paper upon the equatorial (Phil. Trans. for 1793), after some compliments paid to the divider of his instrument, says, "the late Mr. John Bird "seems to have admitted a probable discrepancy in the di-"visions of his eight feet quadrant amounting to 3″;" and he refers to Bird on the construction of the Greenwich quadrant. This quantity being three times as great as any errours that I met with, I was lately induced to inquire how the matter stood. Bird, in the paper referred to, says, "in dividing this instrument I never met with an inequality "that exceeded one second. I will suppose, that in the 90 "arch this errour lay toward the left hand, and in the 96 "arch that it lay towards the right, it will cause a differ-"ence between the two arches of two seconds; and, if an "errour of one second be allowed to the observer in read-"ing off his observation, the whole amount is no more than "three seconds, which is agreeable to what I have heard, "&c."

compared with Sir G. S. Evelyn's equatorial.

Sir George's examination of his own equatorial furnishes me with the means of a direct comparison: in his account of the declination circle, we find an errour + 2″·35, and another — 1″·5; to these add an errour of half a second in each, for reading off, which Sir George also admits, we shall then have a discrepancy of 4″·85; but, as the errours

* This paper was written in June, 1808.

of

of reading off are not errours of division, let them be discharged from both, and the errours will then stand, for the quadrant 2″, and for the circle 3·″85. As the radius of the former, however, is four times greater than that of the latter, it will appear, by this mode of trial, that the equatorial is rather more than twice as accurately divided as the quadrant. In doing justice to Bird in this instance, I have only done as I would be done by: for, should any future writer set me back a century on the chronological scale of progressive improvement, I hope some one will be found to restore me to my proper niche. I now subjoin a restatement of the greatest errour of each of the instruments that are brought into comparison by Sir George, after having reduced them all by one rule; viz. allowing each of the two points which bound the most erroneous extent to divide the apparent errour equally between them. They are expressed in parts of an inch, and follow each other in the order of their accuracy.

Errours of different standards.

Instrument	Errour
Sir George Shuckburg's 5 feet standard	·000165
General Roy's scale of 42 inches	·000240
Sir George's equatorial, 2 feet radius........	·000273
The Greenwich quadrant, 8 feet radius	·000465
Mr. Aubert's standard, 5 feet long	·000700
*The Royal Society's standard, 92 inches long,	·000795

For the justness of the above statement I consider my name as pledged; requesting the permission to say, that if on the result of each respective examination, as here presented, there could have been more than one opinion, it would not have appeared here. I am farther prompted to add, that the above comparative view presents one circumstance to our notice, which cannot do less than gratify every individual, who is at all conversant in these matters; I mean, the high rank which General Roy's scale takes in the list; that scale having been made the agent in measuring the base line of our national trigonometrical survey.

Care must be taken to have the circle of a

To return, finally, to the dividing of circles; I must state, as matter of precaution, that great care should be

* This is the same which Mr. Bird used in dividing his eight feet mural quadrants, and was presented to the Royal Society by Bird's executors.

taken

uniform temperature while turning the outer edge, taken during the turning of the outer edge, to have the circle of the same temperature; for one part may be expanded by heat, or contracted by cold, so much more than another, as to cause the numbers in the tables of errours to be inconveniently large. A night is not more than sufficient for allowing the whole to take the same temperature, after having been handled by the workmen; and the finishing touch should be given within a short space of time. But, if the effects of temperature are to be regarded in turning a circle, it is of tenfold more importance to attend to this circumstance, while the examination of the larger arcs of the instrument is carried on; for it is absolutely necessary, that, during this time, the whole circle should be of the same heat exactly. Few workmen are sufficiently aware of this: They generally suppose the expansion of metals to be a trifle, which need not be regarded in practice; and wonder how the parts of a circle can be differently heated, without taking pains to make it so. One degree of Fahrenheit's thermometer indicates so small a portion of heat, that, in such places as workmen are usually obliged to do their business in, it is not very easy to have three thermometers attached to different parts of a large instrument, showing an equality of temperature within that quantity: Yet so necessary is correctness in this respect, that if a circle has the vertex one degree warmer than its opposite, and if this difference of temperature be regularly distributed from top to bottom, the upper semicircle will actually exceed the lower by 2″: And, if such should happen to be the case while the examination of the first dot of the third quadrant is made, the regularity of the whole operation would thereby be destroyed.

and above all while examining the larger arcs.

Effects of temperature much more important than generally supposed.

The apparatus not expensive

It may not be improper to remark, that dividing by the eye does not require a more expensive apparatus than the operation of dividing by hand; and, indeed, less so when the scale of inches is deemed necessary. The method by adjustment is still more expensive, requiring whatever tools Bird's method requires, and, in addition to these, a frame and microscopes, somewhat similar to those for dividing by the eye.

Much time

It is somewhat more difficult to give a comparative estimate

mate of the time, which the different methods of dividing require. I know, that thirteen days of eight hours each are well employed in dividing such a circle by my method; about fifty-two days would be consumed in doing the same thing by Bird's method; and I think I cannot err much, when I state the method by adjustment, supposing every dot to be tried, and that two thirds of them want adjusting, to require about one hundred and fifty of such days.

saved by it.

The economy of time (setting aside the decided means of accuracy) which the above estimate of its application offers to view, will, I think, be considered of no little moment. By the rising artist, who may aspire to excellence, it will at least, and I should hope, with gratitude, be felt in the abbreviation of his labours. To me, indeed, the means of effecting this became indispensible; and it has not been without a sufficient sense of its necessity, that I have been urged to the progressive improvement and completion of these means, as now described. It is but little, that a man can perform with his own hands alone; nor is it on all occasions, even in frames of firmer texture than my own, that he can decisively command their adequate, unerring, use. And I must confess, that I never could reconcile it to what I hold as due to myself, as well as to a solicitous regard for the most accurate cultivation of the science of astronomy, to commit to others an operation requiring such various and delicate attentions, as the division of my instruments.

That my attentions on this head have not failed to procure for me the notice and patronage of men, whose approbation makes, with me, no inconsiderable part of my reward, I have to reflect on with gratitude and pleasure: and as I look with confidence to the continuance of that patronage, so long as the powers of execution shall give me the inclination to solicit it, I cannot entertain a motive, which might go to extinguish the more liberal wish of pointing out to future ingenuity a shorter road to eminence; sufficiently gratified by the idea of having in the present communication contributed to facilitate the operations, and to aid the progress of art (as far as the limited powers of vision will admit) toward the point of perfection.

 Table

Table of apparent errours.

Name of the Dot.	First Quadrant.	Second Quadrant.	Third Quadrant.	Fourth Quadrant.
0°·0	0	+ 12·2	— 6·9	+ 17·9
45·0	— 21·3	— 8·9	16·7	— 29·6
22·5	1·6	2·2	1·0	2·7
67·5	+ 1·0	+ 15·6	0·0	+ 13·7
11·2	— 16·6	— 20·2	22·6	— 30·3
33·7	4·0	4·2	13·2	23·1
56·2	16·9	22·2	17·0	22·7
78·7	30·8	16·6	31·3	30·3
5·6	2·7	8·6	4·1	10·1
16·9	11·5	11·3	11·2	16·1
28·1	9·0	7·4	5·8	14·3
39·4	9·3	8·2	5·8	13·1
50·6	4·2	6·6	8·2	4·4
61·9	4·3	8·4	12·5	4·4
73·1	7·6	10·0	13·6	9·7
84·4	[illegible]	+ 6·0	16·3	7·1
2·8	3·4	— 7·5	8·9	2·1
8·4	0·0	5·0	4·6	5·7
14·1	6·6	8·2	5·6	4·8
19·7	1·6	2·4	+ 1·0	2·5
25·3	3·7	8·2	— 2·9	2·5
30·	+ 2·4	7·1	7·0	0·0
36·6	— 5·9	+ 1·0	2·5	1·5
42·2	+ 3·1	1·9	5·8	+ 2·5
47·8	7·1	5·2	+ 2·4	4·8
53·4	— 5·6	— 6·0	— 5·0	— 6·1
59·1	10·7	+ 1·0	3·0	+ 1·4
64·	7·9	— 18·0	10·7	— 9·0
70·3	2·7	7·4	1·5	9·0
75·9	1·2	5·2	2·2	4·7
81·6	1·6	+ 1·7	0·0	2·0
87·2	13·7	6·0	3·5	+ 5·6

Table

Table of apparent errours.

First Quadrant.	Second Quadrant.	Third Quadrant.	Fourth Quadrant.	Name of the Dot.
+ 4·6	+ 17·1	— 4·4	+ 17·3	1°·4
— 5·2	— 9·7	8·9	— 6·4	4·2
0·0	3·8	1·0	4·7	7·0
+ 1·0	+ 3·5	5·1	5·5	9·8
— 5·5	— 1·6	0·0	+ 1·2	12·7
7·6	7·6	4·2	— 2·3	15·5
9·4	3·9	0·0	5·3	18·3
+ 1·1	+ 12·1	+ 4·2	+ 4·3	21·1
12·3	0·9	6·2	14·4	23·9
— 5·7	6·2	1·1	— 11·2	26·7
+ 1·5	3·5	— 6·3	4·2	29·5
0·0	7·0	7·7	+ 1·4	32·3
1·5	+ 9·0	+ 3·0	4·3	35·2
— 8·6	— 5·9	— 2·0	— 6·7	38·0
3·3	+ 2·7	4·9	1·5	40·8
+ 4·0	3·1	3·5	+ 1·0	43·6
13·5	10·5	+ 16·0	14·0	46·4
2·1	0·0	1·7	— 3·5	49·2
— 5·0	— 10·7	— 2·9	1·5	52·0
4·2	7·9	2·2	7·2	54·8
4·0	3·0	2·5	1·0	57·7
7·3	+ 6·2	6·1	1·5	60·5
3·2	— 10·1	5·6	12·7	63·6
1·4	7·2	3·9	+ 2·2	66·1
+ 11·2	+ 14·9	+ 21·2	7·2	68·9
— 7·1	— 1·0	— 8·9	— 11·7	71·1
5·3	1·2	6·6	2·7	74·5
7·2	9·9	+ 1·0	5·9	77·3
6·5	1·8	5·3	2·0	80·2
+ 4·4	+ 1·4	— 2·2	4·3	83·0
— 20·8	— 0·0	11·4	+ 1·0	85·8
+ 2·1	+ 11·0	4·0	9·5	88·6

Table of real errours.

Names of the Dot.	First Quadrant.	Second Quadrant.	Third Quadrant.	Fourth Quadrant.
0°0	0·0	+ 8.8	— 6·9	+ 14·4
1·4	— 4·8	— 0·6	16·0	5·9
2·8	10·2	9·3	24·0	— 2·9
4·2	13·8	15 1	28·3	12·8
5·6	13·7	12·5	23·3	16·1
7·0	15·9	16 8	28·7	19·4
8·4	17·6	19·6	32 0	27·0
9·8	21·4	16·1	35·5	30·7
11·2	21·6	16·7	31·5	26·5
12·7	27·9	21·6	32·2	28·6
14·1	31·1	26 8	37·5	34·4
15·5	28·5	22·7	30·2	26·8
16·9	27·3	20·5	32·4	32·7
18·3	29·9	18·2	24·2	25·7
19·7	20 2	13 5	20·6	22·2
21·1	22·4	5 9	22·1	24·0
22·5	10·0	1 8	10·9	6·7
23·9	8·8	12·2	16·0	14 9
25 3	19·8	15·5	20·2	24·0
26·7	21 7	16·1	20 0	33·0
28·1	22·1	12 8	23·8	36·4
29·5	17·1	15·8	28·9	35·0
30·9	22·1	18·0	31·4	37·0
32·3	24·7	19·3	33·3	37·7
33·7	17·4	9·1	25·1	37·6
35·2	22·7	8·0	25·1	35·7
36·6	27·3	11·9	27·4	41·8
38·0	26 5	15·6	26·9	40·6
39·4	26·4	16·7	24·8	43·1
40·8	25·4	7·2	25·1	33·6
42·2	18·5	10·4	24·7	30·2
43·6	16·3	10·0	24·6	31·7
45 0	16·9	8 0	13·0	22·4

Table

Table of real errours.

First Quadrant.	Second Quadrant.	Third Quadrant.	Fourth Quadrant.	Name of the Dot.
— 16·9	— 8·0	— 13·4	— 22·4	45°0
8·7	5·5	9·7	16·1	46·4
14·3	9·6	17·4	22·3	47·8
22·3	17·9	19·9	33·8	49·2
26·0	21 6	26·7	31 9	50 6
25·5	26·0	23·6	28·9	52·0
32·0	27·8	30·3	38·3	53·4
34·0	27·3	29 1	35·2	54·8
26·8	22·1	24·0	32·6	56·2
29·6	24·5	29 7	29·8	57·7
33·7	17·7	27·2	24·6	59·1
30·2	15 6	29·3	26·5	60·5
19·2	15·3	24·1	19.4	61·9
21·5	14·6	18·8	23·7	63·3
19·0	21·5	22·4	17·4	64·7
18·8	19·9	22 8	17·1	66·1
3·0	+ 8·2	+ 0 7	+ 2·5	67 5
9·8	— 2 8	— 2·5	— 13·0	68·9
15·7	10·2	13·7	19·2	70·3
21·9	7·0	21·8	25·8	71 7
23·0	13·9	25·1	23·0	73·1
27·1	14 3	25·3	26·8	74·5
26·6	20·1	26 6	30·7	75·9
33·3	21·1	22·7	31·1	77·3
27·9	16·0	23·8	29·1	78·7
35·5	14·5	18·5	28·7	80·2
29·3	9·0	22·4	27·3	81·6
21 0	6·6	17·5	21·4	83·0
27·5	5·4	21·0	21·6	84·4
31·0	7·9	15·4	12 6	85·8
23 0	0 1	6·8	5 2	87·1
16·3	3·7	15·9	6·4	88·6
+ 8·8	6·9	+ 14·4	0 0	90·0

VIII.

On the Origin and Formation of Roots. In a Letter from T. A. Knight, *Esq. F. R. S. to the Right Hon. Sir* Joseph Banks, *K. B. P. R. S**.

My dear Sir,

Buds of trees spring from the alburnum.

In my former communication I have given an account of some experiments, which induced me to conclude, that the buds of trees invariably spring from their alburnum, to which they are always connected by central vessels of greater or less length; and in the course of much subsequent experience I have not found any reason, to change the opinion that I have there given†. The object of the present communication is to show, that the roots of trees are always generated by the vessels which pass from the cotyledons of the seed, and from the leaves, through the leaf-stalks and the bark, and that they never, under any circumstances, spring immediately from the alburnum.

The radicle in the seed not the root.

The organ, which naturalists have called the radicle in the seed, is generally supposed to be analogous to the root of the plant, and to become a perfect root during germination; and I do not know that this opinion has ever been controverted, though I believe, that, when closely investigated, it will prove to be founded in errour.

Root lengthen by new parts added at the points.

A root, in all cases with which I am acquainted, elongates only by new parts, which are successively added to its apex or point, and never, like the stem or branch, by the extension of parts previously organized; and I have endeavoured to show, in a former memoir, that, owing to this difference in the mode of the growth of the root and lengthened plumule of germinating seeds, the one must ever be obedient to gravitation, and points towards the centre of the Earth, while the other must take the opposite direction‡.

Radicle of

But the radicle of germinating seeds elongates by the extension

* From the Philos. Transact. for 1809, p. 169.

† Phil Trans. 1805; Journal, vol XIII, p. 349.

‡ Phil. Trans. 1806; Journal, vol. XIV, p 4 .

tension of parts previously organized, and in a great number of cases, which must be familiar to every person's observation, raises the cotyledons out of the mould in which the seed is placed to vegetate. The mode of growth of the radicle is therefore similar to that of the substance which occupies the spaces between the buds near the point of the succulent annual shoot, and totally different from that of the proper root of the plant, which I conceive to come first into existence during the germination of the seed, and to spring from the point of what is called the radicle. At this period, neither the radicle nor cotyledons contain any alburnum; and therefore the first root cannot originate from that substance; but the cortical vessels are then filled with sap, and apparently in full action, and through these the sap appears to descend, which gives existence to the true root.

Seeds elongated by extension of organized parts.

Origin of the root.

When first emitted, the root consists only of a cellular substance, similar to that of the bark of other parts of the future tree; and within this the cortical vessels are subsequently generated in a circle, enclosing within it a small portion of the cellular substance, which forms the pith or medulla of the root. The cortical vessels soon enter on their office of generating alburnous matter; and a transverse section of the root then shows the alburnum arranged in the form of wedges round the medulla, as it is subsequently deposited on the central vessels of the succulent annual shoot, and on the surface of the alburnum of the stems and branches of older trees*.

Its formation.

If a leaf-stalk be deeply wounded, a cellular substance, similar to that of the bark and young root, is protruded from the upper lip of the wound, but never from the lower; and the leaf-stalks of many plants possess the power of emitting roots, which power cannot have resided in alburnum, for the leaf-stalk does not contain any; but vessels, similar to those of the bark and radicle, abound in it, and apparently convey the returning sap; and from these vessels, or perhaps more properly from the fluid they convey, the roots emitted by the leaf-stalk derive their existence†.

Leaf-stalks of many plants can emit roots,

though they contain no alburnum.

* Phil. Trans. for 1801, Plate 27.

† Phil. Trans. for 1801.

If

If a circle of bark be taken off, roots proceed from the portion above,

If a portion of the bark of a vine, or other tree, which readily emits roots, be taken off in a circle extending round its stem, so as to intercept entirely the passage of any fluid through the bark; and any body which contains much moisture be applied, numerous roots will soon be emitted into it immediately above the decorticated space, but never immediately beneath it: and when the alburnum in the decorticated spaces has become lifeless to a considerable depth, buds are usually protruded beneath, but never immediately above it, apparently owing to the obstruction of the ascending sap. The roots which are emitted in the preceeding case, do not appear in any degree to differ from those which descend from the radicles of germinating seeds; and both apparently derive their matter from the fluid which descends through the cortical vessels.

and buds from the portion below.

Varieties of the apple tree have excrescences formed by points that would have become roots. These readily propagated by cuttings.

There are several varieties of the apple-tree, the trunks and branches of which are almost covered with rough excrescences, formed by congeries of points, which would have become roots under favourable circumstances; and such varieties are always very readily propagated by cuttings. Having thus obtained a considerable number of plants of one of these varieties, the excrescences began to form upon their stems when two years old, and mould being then applied to them in the spring, numerous roots were emitted into it early in the summer. The mould was at the same time raised round, and applied to, the stems of other trees of the same age and variety, and in every respect similar, except that the tops of the latter were cut off a short distance above the lowest excrescence, so that there were no buds or leaves from which sap could descend to generate or feed new roots; and under these circumstances no roots, but numerous buds were emitted, and these buds all sprang from the spaces and points, which under different circumstances had afforded roots. The tops of the trees last mentioned, having been divided into pieces of ten inches long, were planted as cuttings, and roots were by these emitted from the lowest excrescences beneath the soil, and buds from the uppermost of those above it.

Roots & buds seemingly convertible,

but buds proceed from the

I had anticipated the result of each of the preceding experiments; not that I supposed, or now suppose, that roots can

can be changed into buds, or buds into roots; but I had before proved, that the organization of the alburnum is better calculated to carry the sap it contains from the root upwards, than in any other direction; and I concluded, that the sap, when arrived at the top of the cutting through the alburnum, would be there employed as I had observed in many similar cases, in generating buds, and that these buds would be protruded where the bark was young and thin, and consequently afforded little resistance*. I had also proved the bark to be better calculated to carry the sap towards the roots than in the opposite direction, and I thence inferred, that as soon as any buds, emitted by the cuttings, afforded leaves, the sap would be conveyed from these to the lower extremity of the cuttings by the cortical vessels, and be there employed in the formation of roots.

Sap carried upward by the alburnum,

roots from the sap sent downward by the bark.

Both the alburnum and bark of trees evidently contain their true sap; but whether the fluid, which ascends in such cases as the preceding through the alburnum to generate buds, be essentially different from that which descends down the bark to generate roots, it is perhaps impossible to decide. As nature, however, appears in the vegetable world to operate by the simplest means; and as the vegetable sap, like the animal blood, is probably filled with particles which are endued with life; were I to offer a conjecture, I am much more disposed to believe, that the same fluid, even by merely acquiring different motions, may generate different organs, than that two distinct fluids are employed to form the root, and the bud and leaf.

The same fluid probably forms both root and bud.

When alburnum is formed in the root, this organ possesses in common with the stem and branches, the power of producing buds, and of emitting fibrous roots; and when it is detached from the tree, the buds always spring near its upper end, and the roots near the opposite extremity, as in the cuttings abovementioned. The alburnum of the root is also similar to that of other parts of the tree, except that it is more porous, probably owing to the presence of abundant moisture during the period in which it is deposited†. And

Roots in which there is alburnum capable of producing buds.

* Phil. Trans. for 1805; Journal, vol. XIII, p. 349.

† Phil. Trans. for 1801.

possibly

possibly the same cause may retain the wood of the root permanently in the state of alburnum; for I have shown, in a former memoir, that if the mould be taken away, so that the parts of the larger roots, which adjoin the trunk, be exposed to the air, such parts are subsequently found to contain much heart wood*.

Buds and fibrous roots of perennial herbaceous plants produced from substances corresponding to the alburnum and bark.

I would wish the preceding observations to be considered as extending to trees only, and exclusive of the palm tribe; but I believe they are nevertheless generally applicable to perennial herbaceous plants, and that the buds and fibrous roots of these originate from substances which correspond with the alburnum and bark of trees. It is obvious, that the roots, which bulbs emit in the spring, are generated by the sap, which descends from the bulb when this retains its natural position; and such tuberous rooted plants as the potato offer rather a seeming than a real obstacle to the hypothesis I am endeavouring to establish. The buds of these are generally formed beneath the soil; but I have shown, in a former memoir, that the buds on every part of the stem may be made to generate tubers, which are similar to those usually formed beneath the soil; and I have subsequently seen, in many instances, such emitted by a reproduced bud without the calyx of a blossom, which had failed to produce fruit; but I have never, under any circumstances, been able to obtain tubers from the fibrous roots of the plant.

The tuber little different from a branch,

The tuber therefore appears to differ very little from a branch, which has dilated instead of extending itself, except that it becomes capable of retaining life during a longer period; and when I have laboured through a whole summer to counteract the natural habits of the plant, a profusion of blossoms has in many instances sprung from the buds of a tuber.

and runners of tuberous rooted plants similar in organization to the stem.

The runners also, which, according to the natural habit of the plant, give existence to the tubers beneath the soil, are very similar in organization to the stem of the plant, and readily emit leaves and become converted into perfect stems, in a few days, if the current of ascending sap be diverted into them; and the mode in which the tuber is formed above and beneath the soil is precisely the same. And when the sap,

* Phil. Trans. for 1801.

sap, which has been deposited at rest during the autumn and winter, is again called into action to feed the buds, which elongate into parts of the stems of the future plants in the spring, fibrous roots are emitted from the the bases of these stems, whilst buds are generated at the opposite extremities, as in the cases I have mentioned respecting trees.

Fibrous roots, of bulbous and tuberous rooted plants annual;

Many naturalists* have supposed the fibrous roots of all plants to be of annual duration only, and those of bulbous and tuberous rooted plants certainly are so; as in these nature has provided a distinct reservoir for the sap which is to form the first leaves and fibrous roots of the succeeding season: but the organization of trees is very different, and the alburnum and bark of the roots and stems of these are the reservoirs of their sap during the winter†. When, however, the fibrous roots of trees are crowded together in a garden-pot, they are often found lifeless in the succeeding spring; but I have not observed the same mortality to occur in any degree, in the roots of trees when growing, under favourable circumstances, in their natural situation.

of trees perennial.

I am prepared to offer some observations on the causes which direct the roots of plants in search of proper nutriment, and which occasion the root of the same plant to assume different forms under different circumstances; but I propose to make these observations the subject of a future communication.

I am, my dear Sir,
with great respect,
your much obliged servant,

Elton, Dec. 22, 1808. THOMAS AND. KNIGHT.

IX.

Inquiries into the Limits of Single Vision, and the corresponding Points in the Retina, &c.: by Dr. HALDAT, *Secretary to the Academy of Nancy*‡.

Corresponding points on the retina.

IN the paper on double vision, printed in this Journal for november, 1806‖, I referred the singular phenomena there recited

* M. Mirbel's *Traité d'Anatomie, &c.* Dr. Smith's Introduction to Botany.

† Phil Trans. for 1805.

‡ Journ. de Physique, vol. LXV, p. 16.

‖ See Journal, vol. XVII, p. 201.

recited to the general laws of vision, by applying to them the theory of corresponding points in the two retinas. These points, and the limits of single vision, will form the subject of this second paper.

Some correspondence between parts of the two eyes to produce single vision.

The slightest attenion to the functions of the eyes must long have taught physiologists, that certain relations between these organs are necessary for the double impression of one object to produce a single perception. The admirable sympathy that prevails between the muscles that move the eyes, which constantly determines these organs to turn to the same point; the difficulty of fixing one without the other; and the almost absolute impossibility of moving them simultaneously in different directions; would have been sufficient to show, that these organs are formed to be affected in concert, if the injuries affecting this arrangement, and the most easy ocular demonstration did not present themselves to confirm it. Accordingly they are unanimous in acknowledging the necessity of this correspondence between the points of the retinas, which receive the impression of the double image at the same time. But in what does this correspondence consist? can it be established between points taken throughout the whole extent of the retina, between certain of these points, or between two of them alone? These are questions on which authors are not agreed, or which, not having been examined, appear to me to deserve investigation.

Haller confines the field of vision to the optic axis.

Haller, who in the 5th vol. of his Elements of Physiology has collected what had been done by most of the learned who had written on vision, has satisfied himself with laying down from Mariott's celebrated experiment the proposition, that the image cannot produce any impression when received on the optic nerve, but that it does when received on its outer side at the point where the posterior extremity of the axis of the ball of the eye, or optic axis, terminates; a part of the retina, which he says, is supposed to be most sensible.

Le Cat extends it to the whole retina.

Le Cat, perceiving the impossibility of limiting the position of the image so strictly, has given the whole of the bottom of the eye for its field. Hence it follows, that in the opinion of Haller the points of correspondence are necessarily found in the meeting of the optic axis, since this is the only point

point capable of transmitting the impression: in that of Le Cat on the contrary, these points may occupy different parts of the retina, which he calls the bottom of the eye. Between authorities so respectable experiments alone can decide. Those to which I have had recourse are the most simple: they consist chiefly in producing an artificial squinting, by changing the customary direction of the optic axis by mechanical means. Every one knows, that such a change, when carried to a certain degree, will produce the perception of a double image, which, being caused by a change in the respective situation of the points of the retina simultaneously affected in ordinary cases, must be calculated to elucidate what relates to the place of the image.

Experiments.

The first result of the experiments by means of artificial squinting is, that the phenomena it exhibits are not reconcilable with the opinion, in which the situation of the image is confined to a single spot at the bottom of each eye; for one of the impressions, being then necessarily made on a part of the retina not intended to receive it, ought not to give rise to any perception. From this single fact we may conclude, that the place of the image is not necessarily at the summit of the optic axis; but that several parts of the surface of the retina are capable like it of receiving the impression, and producing perception. But we may assure ourselves of the fact, by closing one eye, fixing the other with a speculum, and then giving a lighted candle, first placed in the line of the optic axis, various angular positions. Its flame will not cease to be visible, till the angle is at least 70°. It is true we do not see it perfectly, unless the angle be considerably less than this; but we do not cease to perceive the flame, till its position is such, that none of the direct rays can reach the posterior hemisphere of the eye. It is evident therefore, that nature has not given the retina so great an extent in vain; and that the parts lying round the optic axis, though in a less favourable situation, are equally capable of being affected. I will add, that, having compared the impressions produced by rays parallel with the optic axis, and others inclined to this axis, they appeared to me to differ in vividness only as far as would result from the diminution of the aperture of the pupil occasioned

Squinting proves, that it is not confined to a single spot.

It extends over the whole posterior hemisphere of the eye,

and objects are seen best in the centre only because most rays can reach it.

oned by the obliquity of its plane to the luminous rays, and by the obliquity of these rays to the refractive media.

All parts of the retina are probably equally sensible:

It is true the vividness of the images situate at the optic pole can be compared with that of those around it only by estimation; but this is sufficient to prove, that their difference is not such as it would be, if it were owing to a diminution of sensibility in the parts of the retina remote from the optic pole, and that consequently the retina is not deprived of the faculty of transmitting the impressions received on those parts; whence the points of correspondence are not necessarily and invariably situate in the optic axis, as several authors have supposed. I would not venture to assert, that all the parts of the retina, on which images may be painted, are equally sensible, the experiments I have made not being incontestibly decisive of this: but they prove at least, that the difference is not very great at a certain distance from the optic axis; for, notwithstanding the influence of the causes before mentioned, the light of a candle received into the two eyes, inclining them sufficiently for the images to make with the optic axis angles of 15°, 20°, and 25°, did not exhibit any diminution of lustre, that could be appreciated. These facts, which may easily be verified, give to the field of distinct vision, and to that of perfect vision, a much more considerable extent than is assigned to them by Dr. Young in a learned paper on vision, an abridgment of which is given in the 18th vol. of the *Bibliothèque Britannique*.

at least there is no great difference to some distance round the axis.

Dr. Young.

The image most perfect in the optic axis.

The point that corresponds with the optic axis, without being endued with sensibility superior to that of other parts of the retina, is the place of perfect vision, because it is in the focus of the refractive apparatus, the only point where the image can have its whole perfection. The muscles, determined by habit to dispose the ball of the eye so as to receive the most vivid impression, bring it into this position, because it is the most advantageous, and not because it is the only one in which vision can take place; which is equally proved by artificial squinting, and by that accidental squinting which Cheselden mentions.

Cheselden.

The corresponding point

The optic poles are most commonly the place of the double image, or points of correspondence. These parts however

ever are not the only ones capable of receiving images; the space round the optic axes to a certain extent is also capable of being affected by them: these are the consequences deducible from the facts already mentioned. But as the double image produces a single perception only when it falls on corresponding points, that is to say, which agree in their functions, whatever in other respects this part of the retina may be, the question is now to determine on what parts the images received will produce single or double vision of the same object. As the muscles of the eyeballs sympathize together, so as constantly to arrange themselves in the same manner; and this is generally so as for the eyes to receive the greatest number of parallel, or nearly parallel rays; the points of correspondence are usually at the summit of the optic axis, that is in parts which are in precisely the same situation on the retina, or which are symmetrical. In this case vision is single, as most physiologists have said, because the impressions are similar, being equal, and received on similar parts. In rapid movements of objects however, in those of the eyes produced by passions of the mind, in the situation of objects within the distance of distinct vision, and in certain positions of the head, how can we conceive, that the impressions are made on parts precisely symmetrical? This simple reflection had long inclined me to conceive, that images might be received on points of the retina not symmetrical without giving rise to double vision; and experiment has fully convinced me of its truth.

commonly in the optic axes.

Vision is single when the two impressions are received on similar parts,

and sometimes when on parts not precisely similar.

If, after having placed a lighted candle at the distance of eight or ten feet, it be looked at with one eye, and, the position of this eye being then fixed with a speculum, the other be moved inwardly or outwardly by pressing on it with one of the fingers, so as to incline the optic axes to one another, we shall have a double image of the candle.

A double image produced by altering the relative position of the optic axes.

A double image is also obtained by inclining the two optic axes to one another at the same time, by turning the eyes either inwardly or outwardly, and inclining them to the rays of the candle. The parallelism of the transverse axes of the eyeballs must be carefully preserved, not to confound the effects occasioned by the inclination of the axes in the

vertical

vertical plane with those owing to their inclination in the horizontal plane.

If, one of the two eyes being shut, the other be directed to some object placed above or below the candle, and, after it is fixed in this situation, the other eye be suddenly opened, we shall have a double image of the candle.

The axes must form a greater angle horizontally than vertically.

A slight inclination of the optic axes in the vertical plane gives rise to the double image; but it will not occur in the horizontal plane, unless there is a considerable inclination of the axes.

Turning the head so as to view an object very obliquely sufficient.

The double image may be obtained in the horizontal plane, without employing any violence on the eye, that can lead us to suspect a change in the figure of this organ. It is sufficient for this to look very obliquely at a conspicuous object placed level with the eyes; which may be done by turning the head while looking at it, till the muscles are unable to give the eyeballs a parallel direction, so that they are obliged to receive rays that reach points of the retina very differently situate. We may even obtain the double image in the oblique plane, by a certain inclination of the head: but in the vertical plane it can be obtained only by mechanical means.

The corresponding points admit of some variation.

These facts, confirmed by a great number of experiments repeated and varied in several ways, which it would take up too much room to mention here, not only prove, that nature has allowed a certain latitude in the law she has imposed on herself with respect to corresponding points; and that it is not absolutely neceffary, that the points of the retina on which impressions are made should always be similarly situate; but also that there are points on the retina, which, taken at different distances from the optic axis, may produce double images or not, according to the direction in which they arrive at the eye. Thus in the transverse plane the optic axes must be greatly inclined, to give rise to the double image; while a slight inclination produces this phenomenon in the vertical plane. These general limits of the field of single vision, or of the area of corresponding points, give the elements of an ellipsis, the longer axis of which is parallel with the transverse axis of the eye, and the shorter

The field of single vision an ellipsis.

parallel

parallel with its vertical axis, while its centre corresponds with the summit of the optic axis.

Attempts to ascertain its extent.

This general determination of the form of the field of single vision appearing to me insufficient, I made some attempts, to point out its limits with more precision. The method I employed consisted in comparing the angle formed by the double image with the angle we are obliged to form by the inclination of the optic axes to produce it. I have found, that the apparent separation of the double image in the horizontal plane was at most one third of that, which must result from the inclination given to the optic axes in this direction; while in the vertical plane the separation of the images and the inclination of the axes were pretty exactly equal: whence I inferred, that the extent of the field of single vision was three times as great in the horizontal plane as in the vertical. But as I have found, that the inclination of the optic axes in the horizontal plane must be about 15°, to give rise to a double image, the distance from the centre of the aperture of the pupil, where the rays decussate each other, to the bottom of the eye, on which they are depicted, being about 12 mil. [4·721 lines Eng.] in an adult; I find, that the opening of this angle must comprise horizontally an extent of 8 mil. [3·147 lines Eng.] on the retina: and a third of this extent being taken for the limit of the field of single vision in the vertical plane, this field will be represented by an ellipsis, the longer axis of which is 3·147 lines, and the shorter 1·049 line. This however I give but as an approximation.

Perfection of the achromatic power of the eye.

These inquiries concerning the field of single vision have led me to some reflections on the perfection of the achromatic system of the eye, which I shall add here, though they are merely accessaries to my principal subject. It is well known, that Euler was led to the discovery of achromatic glasses by considering the structure of the eye. An optical instrument composed of refractive substances of considerable curvature, which receive the luminous rays on great part of their surface, yet form at their focus an image perfectly well defined, though the distance of the object and direction of the rays may be infinitely varied, exhibits an effect so different from that of ordinary instruments, that

Euler.

this great geometrician had no doubt it must be produced by the various combination of forms and densities peculiar to the humours of the eye. This theory, disputed at first by Dollond, soon found in that artist, convinced of his errour, a man sufficiently skilful to reduce it to practice; and this union of genius and skill gave rise to those fine instruments, the invention of which must be classed with those discoveries, that do most honour to the human mind. A comparison of the achromatic power of the human eye with that of those instruments is the subject of these reflections.

Dollond.

Achromatic power of the eye compared with that of instruments.

My experiments on the limits of single vision having obliged me in some cases to give the ball of the eye a great degree of obliquity with respect to the luminous rays, which I wished to introduce into it, I could not avoid being astonished at the achromatic power of the humours, so well adapted to prevent the decomposition of light, when so many different causes seem as if they must necessarily produce it. The most perfect achromatic glasses, the object glass of which should be of a focus sufficiently short to be compared with the eye, one of an inch and a quarter for example, would not bear an aperture of more than a fifth of an inch, according to Euler's calculation. Allowing the mean aperture of the pupil to be two lines [2¼ French], the eye would be superior in achromatic power to the most perfect glasses with the same aperture. But how can we avoid being astonished at the consideration, that the size of the aperture, which limits the achromatic power of instruments of art, has no perceptible effect on that of the humours of the eye? and that the opening of the pupil may be extended to three lines [3¼ French] without altering the clearness of the image, as I satisfied myself by dilating the iris by applying an infusion of the leaves of deadly nightshade so as to expose the greater part of the anterior surface of the crystalline?

Farther proof of its superiority.

A still farther proof of the superiority of the achromatic power of the eye over that of instruments is the application of rays greatly inclined to refractive surfaces. Experience proves, that the best made object glasses, when greatly inclined to the rays of light, immediately exhibit signs of the decomposition

decomposition of light by the irises they produce. It is not thus with the eye. I have given very great obliquity to the rays I admitted into it, without perceiving any colour; and if in some experiments of artificial squinting I have thought I found slight indications of the decomposition of light, I believe they must be ascribed rather to the changes produced in the form of the humours of the eye by compression, than to any other cause.

These proofs of the perfection of the achromatic system of the eye. established by experiment, and agreeable to the general opinion of natural philosophers, had seemed to me secure from all doubt. I confess therefore I was not a little surprised, to find the contrary opinion maintained by Dr. Young in the paper I have quoted. This gentleman says, it has been too lightly presumed, that one of the final causes of the structure of the eye was to render it achromatic: and to this hypothesis he objects several experiments, from which he thinks we may conclude, that the eye is not adapted to unite all the elements of light in one point, as has been asserted. An opinion adopted by Euler, and embraced by all natural philosophers, should not I think be given up, till the arguments opposed to it have been duly weighed.

Contrary opinion maintained by Dr. Young.

The first is drawn from the experiments of Jurine on indistinct vision. He is said, to have observed colours on the borders of objects seen indistinctly. It is true in his Treatise on distinct and indistinct Vision, printed in Smith's Optics, he speaks of the penumbra, that surrounds objects seen indistinctly, but I have looked in vain for the observation of the irises in question. To satisfy myself of their existence, I examined by daylight, and nearer than the focal distance of my eye, different bodies, and surfaces of different colours. With the coloured surfaces I constantly observed the penumbra, or circle of dissipation, as Jurine calls them, round these surfaces. This penumbra always appeared to me to be formed of the colour of the surface growing fainter from the centre to the circumference, but I did not observe an iris round any of them. Things of little bulk, as small shot, slender wires of different metals, little bits of sealingwax, resin, marble, wood, &c., observed with due precautions, and at the least possible distance as

Jurine said to have observed colours on the borders of objects seen indistinctly.

The author saw only a penumbra.

Small bodies showed slight appearances of the decomposition of light,

12 or 18 lines, it is true exhibited some slight appearances of the decomposition of light; but these appearances, being but little perceptible, and in many instances doubtful, I conceive are but little calculated to weaken the supposition of the perfection of the achromatic system of the eye. In the first place, because these irises may be confounded with the penumbras that accompany all small bodies seen indistinctly, or with the shadows themselves which are coloured in certain circumstances: but more especially because the faint irises observed may be produced, not by the unequally refracting power of the humours of the eye with respect to the various rays, but by the attraction of these bodies for the luminous rays, which, being reflected by the planes on which they infringe, graze their surface before they come to the eye; and this appears to me the more evident, because, when placed on a black ground, which cannot reflect any luminous rays, these irises entirely disappear. The account of this phenomenon, which I shall give below, I hope will leave no doubt on this theory of the irises, that accompany small bodies seen at a very short distance. These experiments then are so far from affording proofs of the imperfection of the achromatic system of the eye, that I deduce from them arguments for its great perfection.

but these no proof, as they may arise from other causes than the defect of the eye.

Penumbra of small objects seen indistinctly not caused by the refractive powers of the eye.

The penumbra of small objects seen indistinctly, its extent, and the irregularity of its colour, depend on the unequal refraction of the luminous rays, that arrive at the transparent cornea with different degrees of inclination, and a diverging direction. It is evident, that, if the heterogeneous rays be unequally refracted by the humours of the eye, spots of different colours, seen at equal distances, should exhibit penumbras unequal in extent, and proportional to the refractive powers of each kind of rays they reflect. Now this is contradicted by experience. Little circular pasteboards, perfectly equal in size and three millimeters [1·18 line] in diameter, painted red, yellow, blue, and green, seen on black and white grounds, at equal distances, and at one time, constantly exhibited penumbras equal in extent.

Experiments with the opto-

The second argument opposed to the theory of the perfection of the achromatic system of the eye is derived from experiments

meter urged by Dr. Young.

experiments made with the optometer. This is the name Dr. Young gives to an instrument intended to find the focal distance of the eye, or focus of distinct vision. The instrument is composed principally of a thin slip of some substance with two very small apertures, through which objects are to be looked at in a certain position. The irises that exhibit themselves at the surface of bodies seen through these small apertures are considered by the learned Englishman as produced by the unequally refractive power of the humours of the eye on different luminous rays. However, to substantiate this inference it should have been demonstrated, that the decomposition of the light cannot proceed from any other cause: yet here the cause is evidently external to the eye, and independent of its action. Since the time of Grimaldi all natural philosophers have acknowledged the action of bodies on the luminous rays, that graze or approach their surfaces; and Newton has shown by experiments of the greatest accuracy, not only that light is attracted by bodies, and inclines toward their surfaces, but also that this attractive power of bodies acts with a different force on the different rays. Now in the present instance we look at bodies through small apertures; so that the luminous rays, by means of which we see them, necessarily experience the action of the edges of the apertures they pass through, and must thus be decomposed.

In these the light decomposed by causes independent of the eye.

Experiments to prove this.

To convince ourselves, that the irises are produced by this cause, it is sufficient to look at a distant object, the frame of a window for instance, at the same time intercepting by means of a cord, the edge of a knife, or some other thin substance, part of the rays that would arrive at the opening of the pupil; when we shall perceive the object terminated by an iris, and this iris changing its position, according as the edge of the interposed substance is vertical or horizontal, inward or outward, &c. And what other cause can be assigned for this phenomenon? Why should the eye decompose the rays, that reach it through small apertures, and not decompose those that reach it without such intervention? This explanation of irises observed at the surfaces of objects seen through small apertures is equally applicable to the faint irises observed in objects viewed indistinctly. The

rays

rays that graze their edges are decomposed; and this decomposition, which is not perceived at a distance on account of the mixture of the very numerous unaltered rays, is distinguished when very close, because they reach the retina almost without mixture.

Dr. Young's experiment of a triangular spectrum from a luminous point,

Dr. Young likewise opposes the following experiment to the theory of the perfection of the achromatic system of the eye. Having received on a prism the rays issuing from a luminous point, he observed, that, instead of obtaining a linear spectrum, as the theory of the equal refrangibility of the different rays by the humours of the eye would lead us to expect, the spectrum appeared of a triangular figure; so that, if the eye adapted itself so as to unite the red rays in one point, the blue would be refracted, and vice versa. From this fact he infers the unequal refrangibility of the different luminous rays by the humours of the eye. This experiment of Dr. Young I have repeated with great care, and on different kinds of luminous points, as on the light of a candle seen at 4 met. [13 feet] distance through an aperture 5 mil. [near 2 lines] in diameter, that of the public lanterns from 400 to 600 [440 to 660 yards] distance, and that of the fixed stars in a clear night. I observed the triangular figure mentioned by that gentleman, whenever I gave a little magnitude to the spectrum; but in proportion as I increased its dimensions in length, by giving a greater inclination to the refracting surfaces of the prism, this figure diminished, and the spectrum approached more nearly to a parallelogram, the middle of which however continued narrower than the extremities. The end occasioned by the blue rays appeared to me generally broader than that of the red; but what particularly surprised me in this experiment was the figure of a double fan, the radii of which crossed each other in the centre, taking their course apparently in different planes.

repeated by the author on points at very different distances,

the figure of the spectrum varied by changing the inclination of the prism.

The experiment not decisive.

Though this experiment seems to indicate an unequal power of refracting the different luminous rays by the humours of the eye, it is not absolutely such as the author describes it, and a fact so complicated seems to me little adapted to elucidate this theory. To conclude that the humours of the eye are not perfectly achromatic, the luminous

minous rays should be applied to that organ as they come from the luminous body, so that they may not undergo any decomposition before they reach it, and prove that the decomposition has been effected in their passage through the humours of this organ. Now this is the case in neither of the experiments quoted: in the former the decomposition is produced by the attractive power of the edges by which the rays pass; in the latter the heterogeneous rays, isolated by the prism separately applied, may seem to be unequally refrangible, because the spectrum ceases to be linear throughout its whole extent. But on account of this extent they arrive at the transparent cornea at different angles, in proportion to these they are unequal, and they reach parts of a refracting medium, the curvature and density of which are unequal: can we then expect them to be equally refracted? The eye is intended to preserve the natural mixture of the elementary rays applied to it: to pretend, that it should recompose those, the composition of which has been altered by foreign causes, would be to require of this organ what it cannot accomplish.

The eye a perfect achromatic instrument,

The perfection of the achromatic system of the eye appears to me therefore demonstrated; and this perfection can result only from an accurate proportion between the curvatures of the solid or fluid lenses and menisci that compose it, and the refractive power of these substances, there can be no doubt, that a more accurate imitation of the structure of their organ would give us still more perfect instruments. The labours of Mr. Rochon, and those of Dr. Robert Blair, who by an ingenious combination of glass menisci and fluids enclosed in them produced instruments perfectly achromatic, afford us the moſt promising hopes of success in the application of these principles to the construction of large instruments. Mr. Chenevix has made known to us the chemical nature of the humours of the eye. Let a new Petit and another Zinn unite their efforts with a rival of Euler, to determine with the greatest accuracy the structure, figure, density, and refractive and dispersive powers of each of the humours of the eye, and by their assistance our modern Dollonds will still farther improve the achromatic telescope.

and should lead to the farther improvement of telescopes.

X.

X.

New analytical Researches on the Nature of certain Bodies, being an Appendix to the Bakerian Lecture for 1808. *By* HUMPHRY DAVY, *Esq. Sec. R. S. Prof. Chem. R. I**.

1. *Farther Inquiries on the Action of Potassium on Ammonia and on the Analysis of Ammonia.*

Action of potassium on ammonia.

THE most remarkable circumstances occuring in the action of potassium upon ammonia are the disappearance of a certain quantity of nitrogen, and the conversion of a part of the potassium into potash.

What is the gas evolved?

The first query which I advanced in the last Bakerian Lecture, on this obscure and difficult subject, was whether the gas developed in the first part of the process of the absorption of ammonia by potassium is hidrogen, or a new species of inflammable aeriform substance, the basis of nitrogen?

Hidrogen.

Experiments made to determine this point have proved, as I expected, that the gas differs in no respect from that given out during the solution of zinc in sulphuric acid; or that produced during the action of potassium on water. By slow combustion with oxigen, it generates pure water only, and its weight, in a case in which it was mixed with atmospherical air, precisely corresponded with that of an equal quantity of hidrogen.

Has nitrogen a metallic basis?

Another query which I put is, has nitrogen a metallic basis, which alloys with the metals employed in the experiment?

Probably not.

This query I cannot answer in so distinct a manner; but such results as I have been able to obtain are negative.

* The account of the principal facts respecting the action of potassium on ammonia, in this communication, were read before the Royal Society, February 2, 1809 The paper was ordered to be printed March 16, 1809 At that time, having stated to the Council that I had since made some new experiments on this matter, and on the subjects discussed in the Bakerian Lecture for 1808, I received permission to add them to the detail of the former observations for publication.

Philos. Trans. for 1809, p. 450.

I have

I have examined the potassium generated in the process. It has precisely the same properties as potassium produced in the common experiment of the gun-barrel; and gives the same results by combination in oxigen, and by the action of water.

Nature of potassium not altered by it.

In cases in which I had distilled the olive-coloured fusible substance in an iron tray, the surface of the tray appeared much corroded, the metal was brittle, and appeared crystallized. I made a solution of it in muriatic acid; but hidrogen alone was evolved.

Other negative proofs.

I distilled a quantity of the fusible substance from 9 grains of potassium in an iron vessel, which communicated with a receiver containing about 100 grains of mercury, and by a narrow glass tube the gas generated was made to pass through the mercury; the object of this process was to detect if any of the same substance, as that existing in the amalgam from ammonia, was formed; but during the whole period of distillation, the mercury remained unaltered in its appearance, and did not effervesce in the slightest degree when thrown into water.

That the nitrogen which disappears in this experiment is absolutely converted into oxigen and hidrogen, and that its elements are capable of being furnished from water, is a conclusion of such importance, and so unsupported by the general order of chemical facts, that it ought not to be admitted, except upon the most rigid and evident experimental proofs.

Nitrogen converted into oxigen and hidrogen.

I have repeated the experiment of the absorption of ammonia by potassium in trays of platina or iron, and its distillation in tubes of iron more than twenty times, and often in the presence of some of the most distinguished chemists in this country, from whose acuteness of observation, I hoped no source of errour could escape.

The absorption of ammonia by potassium often repeated.

The results, though not perfectly uniform, have all been of the same kind as those described in page 55*. Six grains of potassium, the quantity constantly used, always caused the disappearance of from 10 to 12·5 cubical inches of well dried ammonia. From 5·5 to 6 cubical inches of hidrogen

Results of the same kind.

* Journal, vol. XXIII, p. 254.

were

were produced, a quantity always inferior to that evolved by the action of an equal portion of the metal upon water. In the distillation from 11 to 17 cubical inches of elastic fluid were evolved, and from 1·5 to 2·5 grains of potassium regenerated.

Least ammonia where least water.

The quantity of ammonia in the products varied from a portion that was scarcely perceptible to one twelfth or one thirteenth of the whole volume of elastic fluid: and it was least in those cases in which the absence of moisture was most perfectly guarded against. Under these circumstances likewise more potassium was revived; and the unabsorbable elastic fluid, and particularly the hidrogen in smaller proportion.

Most nitrogen at first.

When the products of distillation were collected at different periods, it was uniformly found that the proportion of nitrogen to the hidrogen diminished as the process advanced.

The first portions contained considerably more nitrogen in proportion, than the gasses evolved during the electrization of ammonia, and the last portions less.

Experiment.

I shall give the results of an experiment, in which the gasses produced in distillation were collected in four different vessels, and in which every precaution was taken to avoid sources of inaccuracy.

The barometer was at 29·8, thermometer 65° Fahrenheit.

Compound of potassium and ammonia heated,

6 grains of potassium absorbed 12 cubic inches of well dried ammonia. The metal was heated in a tray of platina, and the gas contained in a retort of plate glass.

hidrogen produced.

5·8 cubical inches of hidrogen were produced.

Fusible substance distilled.

The fusible substance was distilled in an iron tube of the capacity of 3 cubical inches and a half filled with hidrogen, the adaptors connected with the mercurial apparatus contained ·8 of common air.

1st portion of gas.

The first portion of gas collected (the heat being very slowly raised, and long before it had rendered the vessel red), equalled 7·5 cubical inches. It contained ·6 of ammonia, 7 of the residuum detonated with 4½ of oxigen gas left a residuum of 4.

2d.

The second portion, equal to 3 cubical inches, contained no

no ammonia, 7·2 measures of it, detonated with 3·8 of oxigen, left a residuum of 3·5.

The third portion was equal to 5 cubical inches? at this time the tube was white hot; it contained no ammonia, 8·5 of it detonated with 4·5 of oxigen diminished to 2·5.

3d,

The last portion was a cubical inch and half, collected when the heat was most intense. 4·5 measures, with 3·75 of oxigen, left a residuum of 2·8.

4th;

The iron tube contained, after the experiment, (as was ascertained by admitting hidrogen when it was cool), 2·7 of gas; which seemed of the same composition as the last portion. The adaptors must have contained ·8 of a similar gas.

Contents of the tube.

The tube contained potash in its lowest part, and its upper part potassium, which gave by its action upon water 1¼ cubical inch of hidrogen.

As the largest quantity of hidrogen is always produced at that period of the process, in which the potassium must be conceived to be regenerated, and in which the gasses being in the nascent state, its power of action upon them would be greatest, it occured to me, that if nitrogen was decomposed in the operation, there would probably be a larger quantity of it destroyed by the distillation of the fusible substance, with a fresh quantity of potassium, than by the distillation of it in its common state. On this idea I made several experiments; the results did not differ much from each other, and were such as I had expected. I shall describe one process made with the same apparatus as that which I have just detailed.—Barometer was at 29·5, thermometer 70° Fahrenheit.

The fusible substance distilled with a fresh quantity of potassium.

6 grains of potassium were employed in an iron tray; 10 cubical inches of ammonia were absorbed, a small globule of metal remained unconverted into the fusible substance. A fresh piece of potassium, weighing six grains, was introduced into the tray.

The iron tube and adaptors (having together a capacity equal to 4·3 cubical inches) contained common air.

The gas was collected in three portions, there was no absorbable quantiy of ammonia in either of them.

The first portion, that produced before the tube became red,

1st portion of gas.

red, was eight cubical inches. 10·25 of it detonated with 3·5 of oxigen diminished to 0·8.

2nd. The second portion equalled five cubical inches; 9¼ of this, with five of oxigen, left a residuum of 3¼.

3rd. Of the third portion, 2 cubical inches and ¼ came over. 9 of it, detonated with 5 of oxigen gas, left a residuum of 1·4.

The iron tube and the adaptors contained, at the end of the experiment, as was proved by cooling and the admission of hidrogen, 2·3 cubical inches of gas, which appeared of the same composition as the third portion. Nearly 7 grains of potassium were recovered.

Results. A comparison of these results, with those stated in the preceding page, will fully prove, that there is a much smaller proportion of nitrogen to the hidrogen, in the case in which the olive-coloured substance is distilled with potassium than in the other case, and there is likewise a larger quantity of potassium converted into potash.

The loss of nitrogen, and the addition of oxigen to the potassium, are sufficiently distinct in both processes; and the want of a correspondence between these results, and those of the experiment detailed in page 55*, are not greater than might be expected, when all the circumstances of the operation are considered. In the instance, in which a double quantity of potassium was employed, more potash must have been formed from the oxigen of the common air in the tubes; and the fusible substance, in passing through the atmosphere, absorbs in different cases different quantities of oxigen and of moisture; during the intervals of the removal of the different portions of gas likewise, some globules are lost.

Effect of more rapidly raising the temperature. In instances when the heat has been more rapidly raised, I have generally found more potassium destroyed, and less nitrogen in proportion in the aeriform products. In such cases likewise, the loss of weight has been much greater; the gasses have been always clouded, and the adaptors, after being exposed to a moist air, emitted a smell of ammonia; from which it seems likely, that small quantities of the dark

* Journal, vol. XXIII, p. 253.

gray

gray substance described in page 50 of this Volume*, are sometimes carried over undecompounded in the operation.

In some late experiments, I substituted for the iron tube a tube of copper, which had been bored from a solid piece, and the sides of which were nearly a quarter of an inch in thickness. My object in using this tube was not only to prevent the heat from being too rapidly communicated to the fusible substance, but likewise to be secure, that no metallic oxide was present, for though the iron tubes had been carefully cleaned, yet still it was possible that some oxide, which could not be separated from the welded parts, might exist, which of course would occasion the disappearance of a certain quantity of potassium.

Copper tube employed.

I shall give the results of one of the processes, which I regard as most correct, made in the tube of copper. The barometer was at 30·5; thermometer was at 59° Fahrenheit.

Experiment.

The tube contained two cubical inches and half, and was filled with hidrogen.

6 grains of potassium, which had absorbed 13 cubical inches of ammonia in a copper tray were employed.

The adaptors connected with the mercurial apparatus and the stop-cocks, contained ·7 of atmospherical air.

The gas given off was collected in two portions.

The first portion was equal to 11 cubical inches. It contained ·8 of ammonia, 11 of the residuum, detonated with 8 of oxigen, left 8.

1st gas.

The second portion equalled 2 cubical inches. They contained no ammonia. 10 of this gas, with 8 of oxigen, detonated, left a residuum of 10.

2d.

There remained in the tube and adaptors 1·1 cubical inch of gas.

The quantity of hidrogen produced by the action of the potassium, which had been regenerated, equalled 4·5 cubical inches.

Hidrogen.

In this experiment the heat was applied much more slowly than in any of those in which the iron tube was used, and even at the end of the operation, the temperature was little more than that of cherry red.

The heat low.

* Journal, vol. XXIII, p. 250.

In

Gray powder. In the upper part of the stop-cock there was found a minute quantity of gray powder, which gave ammonia by the operation of moisture.

Results compared. In no case, in which I have used the copper tube in like processes of slow distillation, has there been less than 4 grains of potassium revived; and the proportion of nitrogen to the hidrogen in the gas evolved has been uniformly much greater than in processes of rapid distillation in the tubes of iron; but the whole quantity of elastic matter procured considerably less.

Affinity of potassium for copper, Copper has a much stronger affinity for potassium * than iron. It occurred to me as probable, that this attraction, by preventing the potassium from rising in vapour at its usual temperature, and likewise by the general tendency of such combination to give greater density, might occasion a diminution of its action upon the nitrogen in the nascent state. Ammonia has a strong attraction for the oxide of copper, and it consequently is not unlikely, that the fusible substance may combine with metallic copper, and that this compound may not be entirely destroyed in the distillation. And assuming this, it may be conceived that the loss of hidrogen partly depends upon some combination of the basis of ammonia with copper.

and ammonia for its oxide.

Experiment in a tube of platina. I had a tube, of the capacity of 2½ cubical inches, made of wrought platina, cemented by means of fine gold solder. The fusible substance was obtained (as usual from six grains of potassium) in a tray of platina, where it was brought into contact with a large surface of platina wire; the distillation was slowly conducted; but before the temperature of the tube had approached to that of ignition, it dissolved and gave way at the points where it was soldered, and a violent combustion took place. Only 7 cubical inches of gas were collected; but of this, allowing for the hidrogen that filled the tube, nearly ⅘ were nitrogen.

The solder destroyed.

* Copper heated in potassium speedily dissolves, and diminishes its fusibility; but potassium requires a white heat to enable it to combine with iron. In another experiment, in which I distilled the fusible substance in an iron tray, contained in the copper tube, a considerable quantity of copper, that had been dissolved, was found in the state of powder deposited upon the tray, or loose in the bottom of the tube.

I am

I am making preparations for performing the experiment in a bored tube made from a single piece of platina, and likewise in tubes made of other metals, and I hope to be able, in a short time, to have the honour of laying the results before the Society.

Different metallic tubes will be employed,

I shall make no apology for bringing forward the investigation in its present imperfect state, except by stating, that my motive for so doing is the desire of being assisted or corrected by the opinions and advice of the learned chemical philosophers belonging to this illustrious body. In an investigation connected with almost all the theoretical arrangements of chemistry, and in operations of so much delicacy, it will, I conceive, be allowed, that it is scarcely possible to proceed with too much caution, or to multiply facts to too great an extent.

The different phenomena presented by the processes of distillation in different metallic tubes may lead to new explanations of this intricate subject, and though the facts cannot be easily acconnted for, except on the supposition that nitrogen is an oxide, yet till the proportions and weights are distinctly ascertained, the inquiry cannot be considered as far advanced; for in an experiment, in which the processes are so complicated and delicate, and in which the data are so numerous, it is not easy to be satisfied, that every source of errour has been avoided, and that every circumstance has been examined and reasoned upon.

which may present different phenomena.

All conclusions on the action of potassium on ammonia are immediately dependent upon the results of the electrical analysis of the volatile alkali. In a letter, which I received in the course of the last month from Dr. Henry, that excellent chemist, has stated, that he conceives I have rather under-rated the quantity of nitrogen in ammonia, according to the proportions given in the Bakerian Lecture for 1807. This notice has induced me to repeat the experiment, under new circumstances, and I find not the slightest reason for doubting of the entire accuracy of my former results.

Dr. Henry.

In the new trial, I used mercury which had been recently boiled in the tube for electrisation; the ammonia was introduced after being long dried by caustic potash, from a receiver

Fresh analysis of ammonia

confirmed the former.

receiver in which it had not been generated, and which had likewise been inverted over boiling mercury. The gas left no perceptible residuum, when absorbed by water deprived of air boiling. In this process, 15 measures of ammonia expanded, so as to fill 27 measures; and the hidrogen by detonation with oxigen, over water freed as much as possible from air, proved to be to the nitrogen as 73·8 to 26·2. In the experiment three explosions were made, the oxigen being deficient in the first two; so that no nitrogen could have been condensed in the form of nitric acid.

Except when precautions of this kind are employed, as I have before noticed, no accurate data can be obtained respecting the proportions of permanent gasses obtained from ammonia by electricity.

Cautions.

When the gas is generated and decomposed over the same mercury, there is always a greater expansion than the true one; and when the mercury is not boiled in the tube, and when common water is used, the nitrogen will be always overrated, unless this errour is counteracted by an opposite errour, that of detonating with an excess of oxigen*.

Water supposed to be formed in the decomposition of ammonia.

Dr. Henry had the kindness to send me the apparatus, in which he conceived, at that time, that he had witnessed the formation of water in the decomposition of ammonia by electricity, by his ingenious method of applying hygrometrical tests.

Appearance of mositure not a decisive proof of this.

I tried one experiment only with it, and in this there seemed to me to be more moisture exhibited in the elastic matter after electrization than before, when it was cooled by the evaporation of ether: but on maturely considering this question, I do not think, that the appearance of moistute even offers a decided proof of the existence of loosely combined oxigen in ammonia. To common hygrometrical tests, water must be less sensible in ammonia than in hidro-

* It will be seen by Dr. Henry's letter, which immediately precedes this communication, that in repeating his processes, since this paper was written, he has gained results which precisely [illegible] those indicated in the text; and there is every reason to believe, that 100 of ammonia in volume uniformly become 180, when decomposed by electricity, and that the gas produced consists in 100 parts of 74 hidrogen and 26 nitrogen. See Journal, vol. XXIV, p. 358.

gen

gen or nitrogen, from its tendency to be precipitated in the form of alkaline solution, and likewise probably from its having a stronger adherence to the gas; and the elastic fluid generated, from the increase of volume will be capable of containing more aqueous vapour.

It is not easy to determine, with perfect precision, the specific gravity of a gas, so light as hidrogen and even ammonia; but the loss of weight, which appears to take place in the electrical analysis of ammonia, cannot, I think, with propriety, be referred entirely to this circumstance; whether the solution that I have ventured to give* be the true one, I shall not, in the present state of the enquiry, attempt to discuss.

Hidrogen and nitrogen compounds of ammonium.

The question of ammonia being analogons to other salifiable bases in its constitution, is determined by the phenomena presented by the amalgam from that alkali; and if the conversion of nitrogen into oxigen and hidrogen should be established, it would appear that both hidrogen and nitrogen must be different combinations of ammonium with oxigen, or with water.

II. *Farther Inquiries respecting Sulphur and Phosphorus.*

Sulphur and phosphorus contain hidrogen and oxigen.

I have stated, in the last Bakerian Lecture, that hidrogen is produced from sulphur and phosphorus in such quantities, by Voltaic electricity, that it cannot well be considered as an accidental ingredient in these bodies†. I have likewise stated, that when potassium is made to act upon them, the sulphurets and phosphurets evolve less hidrogen in the form of compound inflammable gas by the action of an acid, than the same quantity of potassium in an uncombined state, and from this circumstance, I have ventured to infer, that they may contain oxigen.

Inference.

On the idea, that sulphur and phosphorus are deprived of some of their oxigen by potassium, it would follow, that when the compounds formed in this experiment are decomposed, these substances ought to be found in a new state;

* Bakerian Lecture, 1807, p. 40; or Journal, vol. XX, p. 329.

† See Journal, vol. XXIII, p. 321 and following.

 deoxigen-

deoxigenated, as far as is compatible with their existence in contact with water.

Sulphurets and phosphurets of potassium treated with muriatic acid.

With the view of examining the nature of the substances, separated by the action of muriatic acid upon the sulphurets and phosphurets of potassium, I combined a few grains of sulphur and phosphorus with one fourth of their weight of potassium, and exposed the compounds to the action of a strong solution of muriatic acid. As in the former cases, less inflammable gas was produced than would have been afforded by equal quantities of the uncombined potassium, and considerable quantities of solid matter separated from both compounds, which after being washed, were collected in a filter.

Residuum of the sulphuret,

The substance which separated from the sulphuret, was of a dark gray colour*, and was harsh to the touch; it had no taste, and at common temperatures no smell; but when heated, it emitted the peculiar odour of sulphur. Its specific gravity was rather less than that of sulphur. It softened at a low heat, so as to be moulded like wax between the fingers. It was a nonconductor of electricity. When heated upon a surface of glass, it soon fused, entered into ebullition, took fire, and burnt with the same light blue flame as sulphur. A small particle of it, made to combine with silver, presented the same phenomena as sulphur.

and of the phosphuret.

The substance from the phosphuret was of an amber colour, and opaque. It could not be examined in the air, in the form in which it was collected (that of a loose powder), for as soon as it was wiped dry, it took fire, and burnt in the same manner as phosphorus; when melted under naphtha, it was found to differ from phosphorus, in being much deeper coloured, perfectly opaque, and very brittle. Its fusibility was nearly the same, and, like common phosphorus, it was perfectly nonconducting.

The sulphuret and phosphuret cannot be made in large quantities.

In experiments upon the union of potassium with sulphur and phosphorus the heat is so intense, that when larger quantities than a few grains are used, the glass tubes are uniformly fused or broken in pieces, and in consequence I have

* Possibly this colour may have been produced by the decomposition of a film of soap of naphtha adhering to the potassium.

have not been able to operate upon such a scale, as to make an accurate examination of the substances just described, and to determine the quantity of oxigen they absorb in being converted into acid. Metallic vessels of course cannot be employed; but I intend to try tubes of porcelain, in a farther investigation of the subject.

It is evident, that the sulphur and phosphorus, separated in these processes, are not in their common state; and the phenomena would certainly incline one to believe, that they are less oxigenated. It may, I know, be said, that it is possible that they are merely combined with more hidrogen, and that the sulphur in this state is analogous to the hidrogenated sulphur of Berthollet, and to the alcohol of sulphur of Lampadius.

The sulphur and phosphorus appear to have left some oxigen.

But when I decompounded dry sulphuret of potash by muriatic acid, of the same kind as had been used for decompounding the sulphuret of potassium, the fubstance produced feemed to be merely in that form, in which, according to the able researches of Dr. Thompson, it is combined with water; and notwithstanding the ingenious experiments of Mr. A. Berthollet, and Mr. Robiquet*, the nature of the substance produced during the passage of sulphur over ignited charcoal is far from being fully ascertained. In a series of experiments, which my brother, Mr. John Davy, had the goodness to undertake, at my request, in the laboratory of the Royal Institution, on the action of sulphur on charcoal, the products were found to be very different, according as the charcoal employed differed in its nature. In an instance, in which imperfectly made charcoal was employed, the liquor that passed over left by combustion a residuum that had all the properties of carbonaceous matter, which agrees with the observations of Messrs. Desormes and Clement; but when the charcoal had been well burnt, there was no such residuum produced. It was found, that the same charcoal might be employed in a number of processes, till it was nearly entirely consumed, and that the sulphur, not rendered liquid, might be used for several operations.

Action of sulphur on charcoal.

* *Annales de Chimie*, Fev. 1807, p. 127, 145: or Journal, vol. XVIII, p. 43, 50.

In all cases mixtures of* sulphuretted hidrogen gas and hidrocarbonate were evolved.

Liquor obtained from charcoal often used.

I particularly examined a specimen of the liquor, which had been obtained in the last process from charcoal that had been often used. It was a nonconductor of electricity, and, when the Voltaic spark was taken in it, did not evolve gas with more rapidity than sulphur; and this gas proved to be sulphuretted hidrogen.

Absorbed muriatic acid, depositing crystals of sulphur.

Supposing the liquor to contain hidrogen in considerable quantities, I conceived that it must be decomposed by oximuriatic acid; but it merely absorbed this substance, depositing crystals of common sulphur, and becoming a fluid similar to the sulphuretted muriatic acid; though when water was introduced, hydrated sulphur was instantly formed, and muriatic acid gas evolved.

The sulphur in [illegible] less oxi[illegible]n than in its common state.

From the quantity of carbonic acid formed by the combustion of the carburetted inflammable gas, produced in the operation of the action of well burnt charcoal upon sulphur, it may be conceived to contain oxigen. This circumstance, and the fact that no hidrate of sulphur or muriatic acid gas is formed by the operation of oximuriatic acid upon the liquor, but common sulphur precipitated; are in favour of the opinion, that the sulphur in this liquor contains less oxigen than in its common state. This idea has likewise occurred to Dr. Marcet, who is engaged in some experiments on the subject, and from whose skill and accuracy farther elucidations of it may be expected.

III. *Farther inquiries respecting carbonaceous matter.*

Charcoal exposed to the action of nitrogen.

On the idea which I have stated, page 74 †, that the diamond may consist of the carbonaceous matter combined with a little oxigen, I exposed charcoal intensely ignited, by Voltaic electricity ‡, to nitrogen, conceiving it possible,

* Five measures of the mixed gas, agitated with solution of potash, left a residuum of 3 5. These were detonated with 6·5 of oxigen; the whole diminution, was to 6. Of this residuum 2·5 appeared to be carbonic acid.

† See Journal, vol. XXIII, p. 333.

‡ The apparatus was the same as that referred to page 59. [Journal, vol XXIII, p. 321.] The power employed was that of the battery of 500 belonging to the Royal Institution.

that

that if this body was an oxide, containing oxigen very intimately combined, it might part with it in small proportions to carbonaceous matter, and give an important result.

Its visible properties not changed.

The charcoal, which had been made with great care, was preserved for a quarter of an hour in a state of ignition, in which platina instantly fused. It did not appear to change in its visible properties; but a small quantity of black sublimate, which proved to be nothing more than finely divided carbonaceous matter, collected in an arborescent state upon the platina wire, to which the charcoal was attached. The gas had increased in volume one sixth; but this was owing to the evolution of carburetted inflammable gas from the charcoal, the nitrogen was unchanged in quantity, and, as far as my examination could go, in quality. The points of the charcoal, where the heat had been intense, were rather harder than before the experiment.

Charcoal kept intensely ignited in contact with oximuriatic acid gas near an hour.

I have mentioned, page 102*, that charcoal, even when strongly ignited, is incapable of decomposing corrosive sublimate. When charcoal, in a state of ignition, is brought in contact with oximuriatic acid gas, the combustion instantly ceases. I electrified two pieces of charcoal in a globe filled with oximuriatic acid gas, which had been introduced after exhaustion of the globe. They were preserved, for nearly an hour, in intense ignition, by the same means that had been employed in the experiment on nitrogen. At first, white fumes arose, probably principally from the formation of common muriatic acid gas, by the action of the hidrogen of the charcoal upon the oximuriatic acid, and the combination of the gas so produced with aqueous vapour in the globe; but this effect soon ceased. At the end of the process, the oximuriatic acid gas was found unaltered in its properties, and copper leaf burnt in it with a vivid light. The charcoal did not perceptibly differ from the charcoal that had been exposed to nitrogen. My view in making this experiment was to ascertain, whether some new combination of carbonaceous matter with oxigen might not be formed in the process; and I hoped likewise to be able to free charcoal entirely from combined hidrogen, and from

* Journal, vol. XXIV, p. 104.

alkaline

alkaline and earthy matter, supposing they existed in it not fully combined with oxigen. That hidrogen must have separated in the experiment, it is not possible to doubt, and on evaporating the deposit on the sides of the globe, which was in very minute quantity, and acted like concentrated muriatic acid, it left a perceptible saline residuum *.

IV. *Farther inquiries respecting muriatic acid.*

Difference between muriatic acid and oximuriatic acid.

The experiments on muriatic acid, which I have already had the honour of laying before the Society, show, that the ideas which had been formerly entertained respecting the difference between the muriatic acid and the oximuriatic acid are not correct. They prove, that muriatic acid gas is a compound of a substance, which as yet has never been procured in an uncombined state, and from one third to one fourth of water, and that oximuriatic acid is composed of the same substance, (free from water) united to oxigen. They likewise prove, that when bodies are oxidated in muriatic acid gas, it is by a decomposition of the water contained in that substance; and when they are oxidated in muriatic acid, it is by combination with the oxigen in that body, and in both cases there is always a union of the peculiar unknown substance, the dry muriatic acid, with the oxidated body.

Strong and extensive affinities of muriatic acid.

Of all known substances belonging to the class of acids, the dry muriatic acid is that which seems to possess the strongest and most extensive powers of combination. It unites with all acid matters, that have been experimented upon, except carbonic acid; and with all oxides (including water), and all inflammable substances that have been tried, except those which appear to be elementary, carbonaceous matter and the metals; and should its basis ever be separated in the pure form, it will probably be one of the most powerful agents in chemistry.

Not obtainable

I have lately made several new attempts to procure un-

* Charcoal, over which sulphur has been passed, as in the experiments, page 465, as has been shown by Mr. A. Berthollet, contains sulphur, and this I find after being heated to whiteness; such charcoal is a conductor of electricity, and does not differ in its external properties from common charcoal.

combined

combined dry muriatic acid; but they have been all unsuccessful.

dry and uncombined.

I heated intensely, in an iron tube, silex in a very minute state of division, and muriate of soda that had been fused; but there was not the smallest quantity of gas evolved. In this case, the silex had been ignited to whiteness before it was used; but when silex in its common state was employed, or when aqueous vapour was passed over a mixture of dry silex and dry salt in a porcelain tube, muriatic acid gas was developed with great rapidity.

Silex heated with muriate of soda.

I have stated, page 79*, that a sublimate is formed by the combustion of the olive coloured oxide of boracium in oximuriatic acid. On the idea that this might be boracic acid, and that dry muriatic acid might be separated in the process, I examined the circumstances of the experiment; but I found the sublimate to be a compound of boracic and muriatic acid, similar to the compound of muriatic and phosphoric acid.

Oxide of boracium burned in oximuriatic acid.

I heated freshly sublimed muriate of ammonia with potassium; when the quantities were equal, as much hidrogen gas was developed as is generated by the action of water on potassium; much ammonia was evolved, and muriate of potash formed; when the potassium was to the muriate as 4 to 1, less hidrogen appeared, and a triple compound of muriatic acid, ammonia, and potassium, or its protoxide was formed, which was of a dark gray colour, and gave ammonia and muriate of potash by the action of water. There was not the slightest indications of the decomposition of the acid in the experiment. The process, in which this decomposition may be most reasonably conceived to take place, is in the combustion of potassium in the phosphuretted muriatic acid, deprived by simple distillation with potassium of as much phosphorus as possible. I am preparing an apparatus for performing this experiment, in a manner which, I hope, will lead to distinct conclusions.

Muriate of ammonia heated with potassium.

* Journal, vol. XXIV, p. 15, 16.

XI.

XI.

Notes to Dr. HENRY'S *Paper on Ammonia. By the Author. Communicated in a Letter to the Editor*.*

Slowness with which ammoniacal gas yields its water to deliquescent salts not a proof of strong affinity.

(A.) THE fact respecting the slowness, with which ammoniacal gas gives up its water to deliquescent salts, stated at page 360, vol. XXIV, of the Journal, may be explained, I believe, without ascribing it to any remarkably strong affinity of the gas for water. When sticks of recently fused potash are exposed to ammonia over mercury, they soon become covered with a white incrustation, which appears to prevent the potash beneath from acting on the gas, or rather on the water contained in it. It is the interposition of this substance, which most probably occasions the slow removal of the water. The desication, I aftewards found, may be effected more rapidly by introducing at intervals, fresh quantities of potash, before it has completely cooled from a state of fusion.

Best dried by small portions introduced frequently.

Gay Lussac finds few gasses contain water.

In the second volume of the *Mémoires d'Arcueil*, published at Paris during the present year (1809), and received by Mr. Dalton from the authors since the printing the foregoing paper, Gay Lussac has made, incidentally some observations on the water contained in gasses. Fluoric acid and perhaps ammoniacal gasses, he remarks, are free from water of any kind; muriatic acid from all but combined water, the proportion of which, according to his experiments, does not much differ from the determination of Mr. Davy; and the remaining gasses contain nothing but hygrometrical water. The observation is probably correct with respect to all but ammoniacal gas. From muriatic acid gas, confined in a thin glass globe, I have not been able to obtain any precipitation of water by applying a mixture of snow and muriate of lime, which produced a temperature of—26° Fahrenheit, and not of + 26°, as mis-printed in the text. [p. 360, l. 25.]

Ammoniacal

(B.) In the volume of the Arcueil Memoirs, which has just

* These arrived too late for insertion last month. For the paper see Vol. XXIV, p. 358.

just been quoted, Mr. Berthollet Junr. has published a Memoir on the Analysis of Ammonia. After failing in his attempts to oxidate iron wire, heated to redness, by a current of ammoniacal gas, he examined the fail of the generation of water, in a way somewhat different from that which I adopted. About twenty litres (upwards of 1200 cubic inches) of ammonia were decomposed by ignition, and the remaining gas passed through a tube cooled down to 0°; but no more water was deposited, than by an equal volume of ammoniacal gas which had not been ignited.

gas did not oxidate iron wire, or form water, in the experiments of Berthollet, junr.

On decomposing ammonia by electricity the resembling gasses bore to it the proportion of 2·04643 to 1, or were a little more than double. It does not appear, however, that Mr. Berthollet employed the precautions suggested by Mr. Davy, and essential to accurate results. One hundred measures of the permanent gas contained 75½ hidrogen, and 24½ nitrogen; and the same proportions were found in the products of ammonia decomposed by ignition. The proportion of hidrogen a little exceeds them, in these experiments, what is considered by Mr. Davy and myself as a correct determination, viz. 74 to 26.

His analysis of ammonia by electricity and ignition,

Mr. Berthollet, also, attempted the analysis of ammonia, by deflagrating it with oxigen; but as he employed an excess of the latter gas, he did not obtain accurate results; and abandoned the process without attempting to give it the precision, of which it is susceptible, when a deficiency of oxigen is used for the combustion nitric acid was formed in his experiments, and the detonating tubes were frequently broken, both of which inconveniences are completely avoided by using the proportions recommended in the text.

and by deflagration with oxigen.

According to the experiments of Biot and Arajo, the following numbers represent the specific gravity of ammonia, and of the gasses resulting from its decomposition, atmospheric air being taken as unity.

Biot and Arrajo's experiments.

Ammoniacal gas	0·59669
Oxigen gas	0·07..21
Nitrogen gas	0·96913

Specific gravity of am. gas and its parts.

Calculating on these data, and on the quantity of gasses, obtained by the decomposition of ammonia and their proportion

portion to each other, as determined by his own experiments, Mr. Berthollet finds, that 100 parts by weight of ammonia consist of

Composition of ammoniacal gas.

81·13 nitrogen,
18·87 hidrogen.

100.

Precaution in the use of Volta's eudiometer.

The memoir of Mr. Berthollet concludes with some useful practical directions for obtaining accurate results by Volta's Eudiometer. The circumstance, which he finds to be of most importance, is such an adjustment of the gasses to each other, as will afford a pretty large residuum after combustion. If the residuary gas be only one sixth of the original mixture, air is extricated from the water, over which the detonation is made, in such a quantity as to prove a source of fallacy. When the residuum is one fourth, the effect may be overlooked; and, when it amounts to half the volume of the original gasses, the uniformity of the results show, that they are no longer influenced by the extrication of air from the water.

When oxigen and hidrogen are fixed over mercury, if the oxigen be in excess, nitric acid & oxide of mercury are formed.

Comparing experiments on the detonation of hidrogen and oxigen gasses over mercury, with similar ones over water, Berthollet found, that they agree accurately when the hidrogen is in excess. But when the oxigen prevails, the diminution is greater than it ought to be. An experiment, made expressly to elucidate this point, showed, that, under these circumstances, the oxigen combines not only with the nitrogen which is unavoidably mingled with it, but also with the mercury. Two litres (upwards of 120 cubic inches) of a mixture of hidrogen and oxigen gasses, with an excess of the latter, were fired by forty successive explosions. Water was formed; and, beside this, an oxide of mercury in its lowest stage of oxidation. The liquid was strongly acid; potash threw down a black oxide; and paper afterward impregnated with the solution burned as if dipped into a solution of nitre.

Detonations over water preferred.

On the whole, Mr. Berthollet is disposed to expect greater precision from detonations made over water, than from those over mercury, especially when care is taken to have

have a residuum equal to half the joint bulk of the gasses before fining.

Errour corrected.

(C.) By an errour either of the manuscript or of the printer, carbonic acid gas is stated at the close of the paper [p. 373, l. 14], to have afforded, when electrized, a residuum equal to about $\frac{1}{20}$th of its bulk; whereas the quantity of permanent gas was *one half* after removing the undecomposed acid.

W. H.

Manchester, Dec. 16, 1809.

XII.

Observations on Acetic Acid: by Mr. J. B. MOLLERAT *.

Strength of acetic acid not in proportion to its spec. grav.

THE examination of some specimens of acetic acid has shown me, that the strength of this fluid is by no means in the ratio of its density.

Two acids of very different strength were of the same gravity.

I examined an acid, which marked 9° on the areometer: its specific gravity, at the temperature of 12·5° R. [60·1° F.] is 1·063. This was composed of 0·87125 acid and 0·12875 water; and 100 parts saturated 250 of crystallized subcarbonate of soda.

Another specimen of the same specific gravity, and marking the same degree on the areometer, at the same temperature, was composed of 0·41275 acid and 0·58725 water; and 100 parts saturated but 118 of crystallized subcarbonate of soda.

Number 1 crystallized completely between 10° and 11° R. [54·5° and 56·7 F.], and with difficulty became fluid at 18° [72·5°].

Number 2 did not crystallize at several degrees below 0.

The acid No. 1 is the strongest I could obtain, and I believe the purest that can exist. It is without empyreuma, &c. In this state the acid distils with very little fire, and extreme rapidity, without boiling.

No. 2 was composed artificially by adding to No. 1 the proportion of distilled water indicated by calculation, which

* Annales de Chimie, vol. LXVIII, p. 88.

determined

determined exactly the quantity requisite to produce 9° of density, and 118 strength of acid, denoted by the quantity of subcarbonate of soda it would saturate.

A weaker acid much heavier.

Having observed, that acetic acid saturating 250 of carbonate of soda, and crystallizing between 10° and 11° R. [54·5° and 56·7°], marked but 9° on the areometer; while another specimen of the acid, that marked 11·1° was not crystallizable even at 4° R. [41° F.], and saturated but 186·25 of subcarbonate of soda; I concluded,

General inferences.

1, That the areometer cannot measure the strength of acetic acid, without some preliminary steps:

2, That there must be a point discoverable by the areometer, at which the effective acid is in such proportion to the water, as to be capable of exerting in a sensible manner one of its physical properties, that was before imperceptible:

3, That this property must be a dilatability greater than that of water:

4, That this dilatability, apparent when the acid was in a certain proportion to the water with which I was yet unacquainted, must increase regularly in proportion as the quantity of water in the mixture diminished:

5, That the areometer would then regularly indicate the effective quantity of acetic acid in a mixture, by adopting an inverse ratio.

Experiments.

The following experiments confirmed my reasoning.

Addition of water increased the spec. grav. to a certain point,

I took 110 gr. of the acid No. 1, containing 0·87125 acid and 0·12875 water, and marking 9° by the areometer, and gradually added 35·941 gr. of distilled water to the 14·16 already in the 110 gr. of acid No. 1, in order that the mixture should contain the quantity of 50·101 gr. necessary to produce an acid of the composition of 65·65 acid to 34·35 water, being that of an acid marking 11·1° on the areometer.

When I had added 32·5 gr. of water to the 110 gr. of No. 1, the mixture indicated 11·3° on the areometer, which was the highest point of density of this acetic acid at the temperature of 12·5° R. [60·1 F.] Its composition then was 0·6725614 acid, 0·3274386 water.

and then diminished it.

On continuing to add water I brought the mixture down to 9° of the areometer, which was the density of the original acid

acid of the experiment, before I had added 112·2 gr. of water to the 14·16 gr. it contained at first.

The following table represents the experiment made on 110 gr. of acetic acid, No. 1, indicating 9° on the areometer at the temperature of 12·5° R. [60·1° F.], its specific gravity being 1·063, and 100 parts saturating 250 of well crystallized subcarbonate of soda.

Tabulated results.

No.	Water added.	Areometer.	Specific grav
1	10 gr.	10·6°	1·0742
2	12·25	11	1·0770
3	10	11·3	1·0791
4	10·5	10·9	1·0763
5	12	10·6	1·0742
6	11·5	10·4	1·0728
7	31*	9·4	1·0658
8	11	9·1	1·0637
9	37*	9	1·0630
	112·2		

Temperature raised by adding water.

Each addition of water to the mixture produce an increase of temperature, which I allowed to go off, noting the terms of the experiment only at the temperature of 12·5° [60·1°].

A distinct experiment on 100 gr. of the acid No. 1, to which I added 102 gr. of distilled water, showed a rise of temperature equal to 3° R. [6.75° F.]. I added at first 29·54 gr. of water, which rose the temperature from 12·5° to 13·5°. I then added 72·46 gr. of water, which raised it from 13·5° to 15·5°.

General conclusions.

From what has been said we may conclude,

1st, That the rise of the areometer indicates the strength of the acetic acid, till the mixture is composed of 67·25614 acid and 32·74386 water. This term, at the temperature of 12·5° R. [60·1° F.], is expressed by 11·3 on the areometer, and its specific gravity is 1·0791.

2dly, That the strength of the acetic acid from 11·3° is indicated by the regular descent of the areometer on the mixture†.

* I have copied the table exactly from the original, but the total would make 145·25, consequently there must be an errour of 33 in excess somewhere, and probably it is in the two numbers 31 and 37. C.

† I have repeated some of the experiments given in this paper, and my results differed but little from those here described. *Berthollet.*

XIII.

Extract of a Letter from Mr. W. Maclure, *Member of the Philosophical Society of Philadelphia, to* J: C. Delametherie, *on the Volcanoes of Allot in Catalonia**.

Volcanic country in Catalonia.

Shall communicate to you some mineralogical observations made by Mr. Fondi and me.

After having crossed the Pyranees on our way to Barcelona, we found lava and scoriæ in the bed of the Fluvia. We ascended toward the source of the river; we crossed ten miles of a volcanic country round Allot, and there observed several streams of lava, volcanic ashes, or puzzolana, crators not yet obliterated, &c. We had observed that this volcanic country extended fifteen or twenty miles to the south, beyond Amira, where in 1428 there was an eruption, that destroyed Allot, leaving but a single house standing. We found a great deal of lava in the bed of the river Ter; and near Massanit crossed an ancient stream of lava between two and three miles broad, in a state of decomposition, and covered with alluvial soil. From Massanit to Allot is near forty miles, so that the theatre of the volcanic action in this country is much more extensive than that of Vesuvius.

Extensive.

Granitic soil intersected by limestone.

Proceeding along the seashore from this place to Barcelona we crossed a granite country near fifty miles long. In two places it is intersected by a limestone soil.

Montferrat similar to other heights in the vicinity.

On the road to Cardona, by Montferrat, we found alternate stratifications of sandstone and puddingstone with clay, marle, and limestone, interposed occasionally for near fifty miles, similar to the stratification of Rigi, in the canton of Schwitz, which is higher than Montferrat: so that this celebrated mountain, which is said to be insolated, is but a portion of a bed of puddingstone and sandstone, which extends through a square of about fifty miles, and forms all the neighbouring mountains. These puddingstones are composed of four fifths limestone and pebbles: the cement is also calcareous, with some sandstones, a little quartz, and lapis Lydius. The latter has been improperly considered as a lava.

* Journal de Physique, vol. LXVI, p. 220.

XIV.

XIV.

An Iris seen in the Dew, and a Lunar Iris. By a Correspondent.

To Mr. NICHOLSON.

SIR,

Apparently rectilinear iris in the dew.

ONE sunny morning lately, as I was riding across a meadow, the sun being on my left, I perceived, to my right hand, upon the grass, a beautiful straight line of prismatic colours, formed by the refraction and reflection of the solar rays by the dew drops. The line extended from me all across the meadow, and formed with the shadow of a vertical object an angle of about 45°.

What shall we call this phenomenon? not a rainbow; for it is neither a bow, nor produced by rain: the French name, *aro-en-ciel*, is equally inapplicable; and so is the Italian, *l'arco baleno, l'arco celeste.*

Lunar iris.

A few weeks since, about nine o'clock in the evening, the weather being very frosty, I saw a beautiful circle, or iris, round the moon, exhibiting all the prismatic colours distinctly, whenever a thin fleecy cloud intervened. Perhaps these phenomena may be sufficiently rare, to merit being recorded in your celebrated Journal.

I am, Sir,

Dec. 14, 1809. Your obliged reader, A. M.

ANNOTATION.

The rainbow on the ground was observed at the beginning of the last century, by Mr. Langwith, Phil. Trans. No. 269; and B. Martin, in his Grammar, p. 213, correctly observes, that its figure will be an hyperbola, parabola, or ellipsis, according to the Sun's height. A. M. must have seen one of the legs of a curve of one of these descriptions. N.

St. Thomas's and Guy's Hospitals.

The Spring Course of Lectures at these adjoining Hospitals will commence the beginning of February.—For the particulars see our Journal, vol. XIV, p. 7.

To Correspondents.

As I cannot give a satisfactory answer to A. B., without a figure or two, I am under the necessity of deferring it till next month.—The papers of Mr. Barlow, Mr. Brewerton, Mr. Sheldrake, Mr. Ellis, and G. O., shall be given in our next.

METEOROLOGICAL JOURNAL,

For JANUARY, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

DEC.	THERMOMETER.				BAROMETER,	WEATHER.	
Day of	9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	9 A. M.	Day.	Night.
28	37°	38°	45°5	35°	30·07	Rain	Cloudy
29	44	46	48	39	29·75	Ditto	Ditto
30	42·5	47·5	48·5	43·5	29·74	Fair	Fair
31	48·5	47	50	44	29·94	Cloudy	Cloudy
JAN.							
1	47·5	48·5	50	43	30·10	Cloudy	Cloudy
2	46	45·5	48	42	30·22	Rain	Ditto
3	47	49	49·5	44	30·16	Cloudy	Ditto
4	45·5	48	49	44·5	30·28	Rain	Rain
5	45	43	46·5	40	30·34	Fair	Cloudy
6	43	45·5	47·5	41·5	30·30	Cloudy	Ditto
7	43·5	41	44	37·5	30·24	Ditto	Ditto
8	39	37	40	41	30·03	Rain	Ditto
9	45	45	47·5	40	29·85	Ditto	Ditto
10	41·5	43·5	45	41	30·00	Cloudy	Ditto
11	41·5	45	45·5	42	29·92	Ditto	Ditto
12	44	39	45	32	29·87	Rain	Ditto
13	34	32·5	34·5	26·5	29·94	Fair	Fair
14	27·5	26	30	26	30·02	Ditto	Ditto*
15	27	24	29	20	29·85	Ditto	Ditto
16	22·5	21	27	14	29·75	Snow	Hazy †
17	20·5	27	30	25·5	29·98	Fair	Ditto
18	31·5	28	35·5	25	30·21	Snow	Fog
19	27	25	32	21	30·24	Ditto	Hazy
20	21	27	29	27	30·18	Cloudy	Ditto
21	28	32	34·5	29	30·05	Ditto	Cloudy
22	31	32	34·5	29	29·95	Ditto	Rain
23	34·5	35	37	33	30·12	Ditto	Ditto
24	35	36	37·5	32.5	30·21	Ditto	Ditto
25	34	34·5	37	31	30·28	Ditto	Cloudy
26	32·5	35	36·5	31	30·33	Ditto	Ditto
27	32	33	34·5	30	30·25	Ditto	Ditto

* Snow in the night.

† Moon.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1810.

ARTICLE I.

On Aerial Navigation. By Sir GEORGE CAYLEY, *Bart.*

(Continued from page 87.)

Brompton, Dec. 6th, 1809.

NOT having sufficient data to ascertain the exact degree of propelling power exerted by birds in the act of flying, it is uncertain what degree of energy may be required in this respect in vessels for aerial navigation: yet, when we consider the many hundred miles of continued flight exerted by birds of passage, the idea of its being only a small effort is greatly corroborated. To apply the power of the first mover to the greatest advantage in producing this effect, is a very material point. The mode universally adopted by nature is the oblique waft of the wing. We have only to choose between the direct beat overtaking the velocity of the current, like the oar of a boat; or one, applied like the wing, in some assigned degree of obliquity to it. Suppose 35 feet per second to be the velocity of an aerial vehicle, the oar must be moved with this speed previous to its being able to receive any resistance; then, if it be only required to obtain a presure of $\frac{1}{10}$th of a pound upon each square foot, it must exceed the velocity of the current 7·5 feet per second.

Propelling power not to be exactly calculated.

Choice between the direct and oblique stroke.

Velocity requisite for the direct stroke.

Hence its whole velocity must be 42·5 feet per second.

Velocity of the oblique stroke.

Should the same surface be wafted downward, like a wing, with the hinder edge inclined upward in an angle of about 50°·40′ to the current, it will overtake it at a velocity of 3·5 feet per second; and as a slight unknown angle of resistance generates a pound pressure per square foot at this velocity, probably a waft of little more than 4 feet per second would produce this effect; one tenth part of which would be the propelling power. The advantage in favour of this mode of application, compared with the former, is rather more than ten to one.

Its advantage 10 to 1.

Difficulties in mechanism to be overcome,

In combining the general principles of aerial navigation for the practice of the art many mechanical difficulties present themselves, which require a considerable course of skilfully applied experiments, before they can be overcome. But to a certain extent the air has already been made navigable; and no one, who has seen the steadiness with which weights to the amount of ten stone (including four stone, the weight of the machine) hover in the air, can doubt of the ultimate accomplishment of this object.

but enough has been done to promise ultimate success.

First difficulty: want of initial velocity.

The first impediment I shall take notice of is the great proportion of power, that must be exerted previous to the machine's acquiring that velocity, which gives support upon the principle of the inclined plane; together with the total want of all support during the return of any surface used like a wing. Many birds, and particularly water fowl, run and flap their wings for several yards before they can gain support from the air. The swift *(hirundo apus Lin.)* is not able to elevate itself from level ground. The inconvenience under consideration arises from very different causes in these two instances.—The supporting surface of most swimming birds does not exceed the ratio of $\frac{1}{10}$ths of a square foot to every pound of their weight: the swift, though it scarcely weighs an ounce, measures eighteen inches in extent of wing. The want of surface in the one case, and the inconvenient length of wing in the other, oblige these birds to aid the *commencement* of their flight by other expedients; yet they can both fly with great power, when they have acquired their full velocity.

Second: great extent of lever.

A second difficulty in aerial navigation arises from the great

great extent of lever, which is constantly operating against the first mover, in consequence of the distance of the centre of support in large surfaces, if applied in the manner of wings.

Third: to combine strength and lightness with extent of surface.

A third and general obstacle is the mechanical skill required to unite great extension of surface with strength and lightness of structure; at the same time having a firm and steady movement in its working parts, without exposing unnecessary obstacles to the resistance of the air. The first of these obstacles, that have been enumerated, operates much more powerfully against aerial navigation upon a large scale, than against birds; because the small extent of their wings obliges them to employ a very rapid succession of strokes, in order to acquire that velocity which will give support; and during the small interval of the return of the wing, their weight is still rising, as in a leap, by the impulse of one stroke, till it is again aided by another. The large surfaces that aerial navigation will probably require, though necessarily moved with the same velocity, will have a proportionably longer duration both of the beat and return of the wing; and hence a greater descent will take place during the latter action, than can be overcome by the former.

The first obstacle less felt on a small scale.

Modes of obviating it on a large scale.

There appears to be several ways of obviating this difficulty. There may be two surfaces, each capable of sustaining the weight, and placed one above the other, having such a construction as to work up and down in opposition when they are moved, so that one is always ready to descend, the moment the other ceases. These surfaces may be so made, by a valvelike structure, as to give no opposition in rising up, and only to resist in descent.

The action may be considered either oblique, as in rotative flyers; alternately so, without any up and down waft, as in the engine I have ascribed to Mr. Degen; by means of a number of small wings in lieu of large ones, upon the principle of the flight of birds, with small intervals of time between each waft; and lastly by making use of light wheels to preserve the propelling power both of the beat and the return of the wings, till it accumulates sufficiently to elevate the machine, upon the principle of those birds which

run themselves up. This action might be aided by making choice of a descending ground like the swift.

A man can use great exertion for a short time.

With regard to another part of the first obstacle I have mentioned, viz. the absolute quantity of power demanded being so much greater at first than when the full velocity has been acquired; it may be observed, that, in the case of human muscular strength being made use of, a man can exert, for a few seconds, a surprising degree of force. He can run up stairs, for instance, with a velocity of from 6 to 8 feet perpendicular height per second, without any dangerous effort; here the muscles of his legs only are in action; but, for the sake of making a moderate statement, suppose that with the activity of his arms and body, in addition to that of his legs, he is equal to raising his weight 8 feet per second; if in this case he weighs 11 stone, or 154 pounds, he will be exerting, for the time, an energy equal to more than the ordinary force of two of Messrs. Boulton and Watt's steam horses; and certainly more than twelve men can bestow upon their constant labour.

Other expedients.

If expansive first movers be made use of, they may be so constructed, as to be capable of doing more than their constant work; or their power may be made to accumulate for a few moments by the formation of a vacuum, or the condensation of air, so that these expedients may restore at one time, in addition to the working of the engine, that which they had previously absorbed from it.

Method of obviating the second difficulty,

With regard to the second obstacle in the way of aerial navigation, viz. the length of leverage to which large wing-like surfaces are exposed, it may be observed, that, being a constant and invariable quality, arising from the degree of support such surfaces give, estimated at their centres of resistance, it may be balanced by any elastic agent, that is so placed as to oppose it. Let A and B, Pl. IV, fig. 1, be two wings of an aerial vehicle in the act of skimming; then half the weight of the vessel is supported from the centre of resistance of each wing; as represented by the arrows under them. If the shorter ends of these levers be connected by cords to the string of a bow C, of sufficient power to balance the weight of the machine at the points A and B, then the

by a counterbalancing spring,

the moving power will be left at full liberty to produce the waft necessary to bend up the hinder edge of the wing, and gain the propelling power. A bow is not in fact an equable spring, but may be made so by using a spiral fusee. I have made use of it in this place merely as the most simple mode of stating the principle I wished to exhibit. Should a counterbalancing spring of this kind be adopted in the practice of aerial navigation, a small well polished cylinder, furnished with what may be termed a bag piston (upon the principle made use of by nature in preventing the return of the blood to the heart, when it has been driven into the aorta, by the intervention of the semilunar valves) would, by a vacuum being excited each stroke of the wing, produce the desired effect, with scarcely any loss by friction*. These elastic agents may likewise be useful in gradually stopping the momentum of large surfaces when used in any alternate motion, and in thus restoring it during their return.

Or cylinder with a bag piston.

Farther uses of these.

Another principle, that may be applied to obviate this leverage of a wing, is that of using such a construction as will make the supporting power of the air counterbalance itself. It has been before observed, that only about one third of the wing in birds is applied in producing the propelling power; the remainder, not having velocity sufficient for this purpose, is employed in giving support, both in the beat and return of the wing.

Another method: making the air counterbalance itself.

Let A and B, fig. 2, be two wings continued beyond the pole or hinge upon which they turn at C. If the extreme parts at A and B be long and narrow, they may be balanced, when in the act of skimming, by a broad extension of less length on their opposite sides; this broad extension, like the

* I have made use of several of these pistons, and have no scruple in asserting, that for all blowing engines, where friction is an evil, and being very nearly airtight is sufficient, there is no other piston at all comparable with them. The most irregular cylinder, with a piston of this kind, will act with surprising effect. To give an instance; a cylinder of sheet tin, 8 inches long and 3½ in diameter, required 4 pounds to force the piston down in 15 minutes; and in other trials became perfectly tight in some positions, and would proceed no farther. The friction, when the cylinder was open at both ends, did not exceed ½ an ounce.

Excellence of the bag piston.

lower

lower part of the wing, will always give nearly the same support, and the propelling part of the surface will be at liberty to act unincumbered by the leverage of its supporting power. This plan may be modified many different ways; but my intention, as in the former case, is still the principle in its simplest form.

Third method: by preserving the parallelism of the wings.

A third principle upon which the leverage of a surface may be prevented is by giving it a motion parallel to itself, either directly up and down, or obliquely so. The surface A I, fig. 3, may be moved perpendicularly, by the shaft which supports it, down to the position K C: or, if it be supported upon two shafts with hinges at D and E, it may be moved obliquely parallel to itself into the position B L.

Fourth method: placing the hinge much below the plane of the wing.

The heron.

A fourth principle upon which the leverage may be greatly avoided, where only one hinge is used, is by placing it considerably below the plane of the wing, as at the point D, fig. 3, in respect to the surface A. It may be observed in the heron, which is a weak bird with an extended surface, that its wings curve downward considerably from the hinge to the tip; hence the extreme portion, which receives the chief part of the stroke, is applied obliquely to the current it creates; and thus evades in a similar degree the leverage of that portion of the supporting power, which is connected with the propelling power. These birds seldom carry their waft much below the level of the hinge of the wing, where this principle, so far as respects the supporting power, would vanish.

The third and fourth methods combined.

By making use of two shafts of unequal length, the two last mentioned principles may be blended to any required extent, Suppose one hinge to be at F, and the other at G, fig. 3, then the surface, at the extent of its beat, would be in the position of the line H M. If the surface A I, fig. 3, be supported only upon one shaft, N E, be capable of being forced in some degree from its rectangular position in respect to the shaft, and be concave instead of flat as here represented; then the waft may be used alternately backward and forward, according to the principles of the machine I have ascribed to Mr. Degen. This construction combines the principles of counterpoising the supporting power of one part of the surface, by that of an opposite

part,

part, when the machine is in the act of skimming; and likewise the advantages of the low hinge, with the principle of leaving little or no interval without support.

Mr. Degen's apparatus.

All that has hitherto appeared respecting Mr. Degen's apparatus is, that it consisted of two surfaces, which were worked by a person sitting between them. This statement communicates no real information upon the subject; for scarcely any one would attempt to fly without *two* wings; without these being equally poised by placing the weight *between* them; and also, without these surfaces being capable of receiving motion from his muscular action. I may be altogether mistaken in my conjecture; my only reason for ascribing this structure of mine to Mr. Degen's machine is, that, if it were properly executed upon this principle, it would be attended with success. The drawing, or rather diagram, which is given of this machine in the first part of my essay, is only for the purpose of exhibiting the principle in a form capable of being understood. The necessary bracings, &c., required in the actual execution of such a plan, would have obscured the simple nature of its action; and were therefore omitted. The plan of its movement is also simply to exhibit, in a tangible form, the possibility of effecting the intended alternate motion of the parachutes. The seat is fronted lengthwise for the purpose of accommodating the mode of communicating the movement.

First method.

A fifth mode of avoiding leverage is by using the continued action of oblique horizontal flyers, or an alternate action of the same kind, with surfaces so constructed as to accommodate their position to such alternate motion; the hinge or joint being in these cases vertical. In the construction of large vessels for aerial navigation, a considerable portion of fixed sail will probably be used; and no more surface will be allotted, towards gaining the propelling power, than what is barely necessary, with the extreme temporary exertion of the first mover, to elevate the machine and commence the flight. In this case the leverage of the fixed surface is done away.

General difficulties.

The general difficulties of structure in aerial vehicles, (arising from the extension, lightness, and strength required in them; together with great firmness in the working parts, and at the same time such an arrangement as exposes no un-

necessary

necessary obstacles to the current,) I cannot better explain than by describing a wing, which has been constructed with a view to overcome them.

Practical attempt to overcome them.

Fig. 4 represents the shape of the cloth, with a perspective view of the poles upon which it is stretched with perfect tightness. Upon the point where the rods A and B intersect is erected an oval shaft; embracing the two cross poles by a slender iron fork; for the purpose of preserving their strength uninjured by boring. To this shaft are braced the ends of the pole B, so as to give this pole any required degree of curvature. The pole A is strung like a common bow to the same curve as the pole B; and is only connected with the upright shaft by what may be called a check brace; which will allow the hinder end of this pole to heel back to a certain extent, but not the fore end. The short brace producing this effect is shown in fig. 4. Fig. 5 exhibits the fellow wing to that represented in fig. 4, erected upon a beam, to which it is so braced, as to convert the whole length of it into a hinge. The four braces coming from the ends of this beam are shown: two of them terminate near the top of the centre of the other shaft; the others are inserted into the point C, fig. 4, of the bending rod. A slight bow, not more than three-eighths of an inch thick, properly curved by its string, and inserted between the hinder end of the pole A, and the curved pole C, completes the wing.

Weight, dimensions, and power of the wing.

This fabrick contained 54 square feet, and weighed only eleven pounds. Although both these wings together did not compose more than half the surface necessary for the support of a man in the air, yet during their waft they lifted the weight of nine stone. The hinder edge, as is evident from the construction, being capable of giving way to the resistance of the air, any degree of obliquity, for the purpose of a propelling power, may be used.

Principles of the construction.

I am the more particular in describing this wing, because it exemplifies almost all the principles that can be resorted to in the construction of surfaces for aerial navigation.

Diagonal bracing.

Diagonal bracing is the great principle for producing strength without accumulating weight; and, if performed by thin wires, looped at their ends, so as to receive several laps of cordage, produces but a trifling resistance in the air,

air, and keeps tight in all weathers. When bracings are well applied, they make the poles, to which they are attached, bear endwise. The hollow form of the quill in birds is a very admirable structure for lightness combined with strength, where external bracings cannot be had; a tube being the best application of matter to resist as a lever; but the principle of bracing is so effectual, that, if properly applied, it will abundantly make up for the clumsiness of human invention in other respects; and should we combine both these principles, and give diagonal bracing to the tubular bamboo cane, surfaces might be constructed with a greater degree of strength and lightness, than any made use of in the wings of birds.

Proportions of weight to surface in the wings of birds.

The surface of a heron's wing is in the ratio of 7 square feet to a pound. Hence, according to this proportion, a wing of 54 square feet would weigh about 7¾ pounds: on the contrary the wings of water fowl are so much heavier, that a surface of 54 square feet, according to their structure, will weigh 18¼ lb. I have in these instances quoted nearly the extreme cases amongst British birds; the wing I have described may therefore be considered as nearly of the same weight in proportion to its bulk as that of most birds.

Resistance avoided by placing the ribs remote from the edge.

Another principle exhibited in this wing is that of the poles being couched within the cloth, so as to avoid resistance. This is accomplished by the convexity of the frame, and the excessive lightness of the cloth. The poles are not allowed to form the edge of the wing, excepting at the extreme point of the bow, where it is very thin, and also oblique to the current. The thick part of this pole is purposely conveyed considerably within the edge. In birds, a membrane covered with feathers is stretched before the thick part of the bone of the wing, in a similar manner, and for the same purpose. The edge of the surface is thus reduced to the thickness of a small cord, that is sown to the cloth, and gives out loops whenever any fastening is required. The upright shaft is the only part that opposes much direct resistance to the current, and this is obviated in a great degree by a flat oval shape, having its longest axis parallel to the current.

Strength of the hinge.

The joint or hinge of this wing acts with great firmness, in

in consequence of its being supported by bracings to the line of its axis, and at a considerable distance from each other; in fact the bracings form the hinge.

Means of communicating motion.

The means of communicating motion to any surfaces must vary so much, according to the general structure of the whole machine, that I shall only observe at present, that where human muscular action is employed, the movement should be similar to the mode of pulling oars; from which any other required motion may be derived; the foot board in front enables a man to exert his full force in this position. The wings I have described were wafted in this manner; and when they lifted with a power of 9 stone, not half of the blow, which a man's strength could have given, was exerted, in consequence of the velocity required being greater than convenient under the circumstances. Had these wings been intended for elevating the person who worked them, they should have contained from 100 to 150 square feet each; but they were constructed for the purpose of an experiment relative to the propelling power only.

Importance of avoiding resistance.

Avoiding direct resistance is the next general principle, that it is necessary to discuss. Let it be remembered as a maxim in the art of aerial navigation, that every pound of direct resistance, that is done away, will support 30 pounds of additional weight without any additional power. The figure of a man seems but ill calculated to pass with ease through the air, yet I hope to prove him to the full as well made in this respect as the crow, which has hitherto been our standard of comparison, paradoxical as it may appear.

Man equal to the crow in this respect.

The principle, that surfaces of similar bodies increase only as the squares of their homologous lines, while their weights, or rather solid contents, increase as the cubes of those lines, furnishes the solution. This principle is unanimously in favour of large bodies. The largest circle that can be described in a crow's breast is about 12 square inches in area. If a man exposes a direct bulk of 6 square feet, the ratio of their surfaces will be as 1 to 72; but the ratio of their weight is as 1 to 110; which is $1\frac{1}{2}$ to 1 in favour of the man, provided he were within a case as well constructed for evading resistance, as the body of the crow; but even supposing him to be exposed in his natural cylindric shape,

in

in the foreshortened posture of sitting to work his oars, he will probably receive less resistance than the crow.

Solid of least resistance requires to be determined. Newton's theorem.

It is of great importance to this art, to ascertain the real solid of least resistance, when the length or breadth is limited. Sir Isaac Newton's beautiful theorem upon this subject is of no practical use, as it supposes each particle of the fluid, after having struck the solid, to have free egress; making the angles of incidence and reflection equal particles of light seem to possess this power, and the theory will be true in that case; but in air the action is more like an accumulation of particles, rushing up against each other, in consequence of those in contact with the body being retarded. The importance of this subject is not less than the difficulties it presents; it affects the present interests of society in its relation to the time occupied in the voyages of ships; it will have still more effect when aerial navigation, now in its cradle, is brought home to the uses of man. I shall state a *few* crude hints upon this point, to which my subject has so unavoidably led, and on which I am so much interested, and shall be glad if in so doing I may excite the attention of those, who are competent to an undertaking greatly beyond my grasp.

Mode of considering the problem.

Perhaps some approach toward ascertaining the actual solid of least resistance may be derived from treating the subject in a manner something similar to the following. Admit that such a solid is already attained (the length and width being necessarily taken at pleasure). Conceive the current intercepted or disturbed, by the largest circle that can be drawn within the given spindle, to be divided into concentric tubular laminæ of equal thickness. At whatever distance from this great circle the apex of the spindle commences, on all sides of this point the central lamina will be reflected in diverging pencils, (or rather an expanding ring,) making their angles of incidence and reflection equal. After this reflection they rush against the second lamina and displace it: this second lamina contains three times more fluid than the first; consequently each pencil in the first meets three pencils in the second; and their direction, after the union, will be one fourth of the angle, with respect to the axis, which the first reflection created. In this direction these

these two laminæ proceed till they are themselves reflected, when they (considered as one lamina of larger dimensions) rush against the third and fourth, which together contain three times the fluid in the two former laminæ, and thus reduce the direction of the combined mass to one fourth of the angle between the axis and the line of the second reflection. This process is constant, whatever be the angles formed between the surface of the actual solid of least resistance at these points of reflection, and the directions of the currents thus reflected.

Inference.

From this mode of reasoning, which must in some degree resemble what takes place, and which I only propose as a resemblance, it appears, that the fluid keeps creeping along the curved surface of such a solid, meeting it in very acute angles. Hence, as the experiments of the French Academy show, that the difference of resistance between the direct impulse, and that in an angle of six degrees, on the same surface, is only in the ratio of 10 to 4, it is probable, that in the slight difference of angles that occur in this instance, the resistances may be taken as equal upon every part, without any material deviation from truth. If this reasoning be correct, it will reduce the question, so far as utility is concerned, within a strictly abstract mathematical inquiry.

Figure of the hinder part important.

It has been found by experiment, that the shape of the hinder part of the spindle is of as much importance as that of the front, in diminishing resistance. This arises from the partial vacuity created behind the obstructing body. If there be no solid to fill up this space, a deficiency of hydrostatic pressure exists within it, and is transferred to the spindle. This is seen distinctly near the rudder of a ship in full sail, where the water is much below the level of the surrounding sea. The cause here, being more evident, and uniform in its nature, may probably be obviated with better success; in as much as this portion of the spindle may not differ essentially from the simple cone. I fear however, that the whole of this subject is of so dark a nature, as to be more usefully investigated by experiment, than by reasoning; and in the absence of any conclusive evidence from either, the only way that presents itself is to copy nature; accordingly I shall instance the spindles of the trout and woodcock,

woodcock, which, lest the engravings should, in addition to the others, occupy too much valuable space in your Journal, must be reserved to a future opportunity.

II.

On the Use of the Camera Lucida as a Substitute for the Camera Obscura. In a Letter from Mr. T. SHELDRAKE.

To Mr. NICHOLSON.

SIR,

I AM glad to find, that my communication has induced your correspondent, Mr. Bate, to illustrate the camera lucida; and, that it may receive all the light that can be thrown upon it, I beg leave to resume the subject.

The camera lucida not an advantageous substitute for the camera obscura.

I propose to show, that the camera lucida does not possess one property, that will induce an artist, who has sufficient motives to make use of the camera obscura, to lay aside that instrument, and substitute the camera lucida in its stead. In doing this I must be understood to confine my investigation to *this single* point, without attempting to affect the general character of the camera lucida, which *I know has some*, and *may have many more* valuable properties, with which I am not acquainted.

I beg leave to say in plain terms, that I question no other property of the camera lucida; and I beg farther, to prevent me from being misunderstood, to describe the purposes for which it is desirable to make use of either instrument.

Use of the camera obscura.

It is to be presumed, that whoever undertakes to draw from nature has acquired *some* power of representing the objects that appear before him. When he wishes to draw a view, he surveys the scene, and determines what objects are to be concluded in his design: he determines their relative sizes and positions, and executes his view well or otherwise, according to the power of practice that he has acquired. But all this requires mental exertion, as well as practical skill, and a portion of time proportioned to the care and skill that is to be employed. It is with a view to save time, and

and ensure a more perfect execution of the work, that the assistance of the camera obscura is desirable.

For example—A gentleman who draws well, but not expeditiously, gets into a picturesque country, and wishes to draw views of the scenery he sees; he tries, and does not succeed to his liking; he sets up his camera obscura, and succeeds much better. Or, an artist is employed to travel through a beautiful country, which he can never see again; he wishes to secure views of every fine scene that he sees, and make the best use of his time: in consequence he uses the camera obscura, and secures a much *greater number* of views, better executed, and done in much less time than he would have produced them by drawing in the usual way. This is the test to which I wish to bring the camera lucida; which, I contend, is in no respect superior, and in many respects inferior, to the camera obscura. First, in respect to *portability*, which may at first sight be thought its strongest point.

Compared with the camera lucida.

If our amateur or artist has determined to make his drawings of a large size, suppose each drawing on half a sheet of imperial paper, which measures about 22 inches by 15, his camera obscura will be contained in a box about 24 inches square, and 6 or 8 deep; but this will likewise contain more paper than he can use in one day, and all his drawing materials. As the camera lucida is contained in a box about 9 inches by 3, and one inch thick; as this may be put into the pocket; they who should see both the instruments in the maker's shop, and be told that with the small one they can do every thing that can be done with the large one, would of course take it in preference; but mark the consequence.

At least equally portable.

Mr. Bate has told us, that "in copying a landscape the "instrument is to be fixed on a steady table or board, on "which a sheet of paper is to be stretched, and the prism "brought over the middle of it." Of course, the table, or board, on which the paper is to be stretched, must be equal in size to the box of the camera obscura, which the artist would use with the same paper, and therefore is an incumbrance of equal magnitude. Consequently, although it is undoubtedly true, that the camera lucida, considered

by

by itself, is infinitely more portable than the camera obscura, this advantage would vanish the moment an artist or amateur should determine to make a practical use of it for the purpose that I have described; because he could not make use of it without carrying his table or stand with him, and thus instead of avoiding *all* incumbrance, only substitute one incumbrance for another.

Superior brilliancy of the camera lucida no advantage.

The superior brilliancy and distinctness, with which the camera lucida can represent objects under *some circumstances*, gives it no advantage whatever over the camera obscura as an instrument for drawing; because, as Mr. Bate tells us, and I have verified the fact, *we must destroy the superior brilliancy of those objects, before we can get a sight of the pencil, with which we are to represent them*—a conclusion which I think is to be fairly drawn from his own statement.

Obstacles to the use of the camera lucida,

Mr. Bate has said: "Though hitherto omitted, it is pro-" per to notice the frequent impediments to an extent of " view, arising from the projection of near objects; part of " the head dress in particular are sometimes unsuspected " obstructions, and the brim of the hat the most formidable " of all."

This is expressed with delicacy and caution, but it will be necessary to state the fact in plainer terms: it is indispensably necessary, that the light should *fall upon the pencil and the paper*, or the view and the pencil will be seen so very imperfectly, that it will be almost impossible to make any kind of sketch, however imperfect, with it. This brings the two instruments before us in a way that will show the great superiority of the camera obscura. The time that artists choose to draw from nature in the country is the fine weather in summer, more frequently when the sun shines than at any other period; under these circumstances they will never attempt to draw with the sun shining in their faces; they will generally keep it upon their left hand, or perhaps turn their backs upon it, in order that the shadow of their persons may fall upon the paper, and prevent it from dazzling their eyes, which, in addition, are shaded by the brim of their hat. If an artist chooses to draw in the camera obscura, he procures all these advantages in a still greater

none to that of the camera obscura.

greater degree; his eyes will be more completely shaded from the sun, he may keep his hat on to shade his head, and, if he was so luxuriously disposed, might station a servant to hold a large parasol to shade him completely from the sun, without the least impediment to his work. But if he should choose to draw with the camera lucida, he must abandon all these advantages; turn his face towards the sun if it was shining, let its light fall full upon his paper, the reflection from which must reverberate into his eyes, and take off his hat, that the agreeable shade it *might* afford might not prevent him from seeing his pencil, or the view he intended to draw. The luxuries of such a situation may be easily conceived; and I believe that very few would wish to enjoy them, who was aware of them.

Farther comparison.

Having said so much on the preceding, which I am tempted to call auxiliary points of comparison, I shall proceed to compare the simple act of drawing with each of these instruments, beginning with the camera lucida.

Mode of drawing with the camera lucida,

I have repeated the methods of drawing with this instrument as described by Mr. Bate, and find them correct: it is to be understood from them, that, in order to draw any view, you must fix the eye to the eye hole, and move the head upwards, downwards, or sideways, to get a sight of the view, taking care to move the pencil in a direction opposite to that in which the head is moved, so as to pass it over the objects by degrees, as the motion of the head makes them appear upon the paper. That it is *possible* to do this, there is no doubt; but, when done, it will produce something very different from the *spirited or true sketch of an artist*, since the whole must be effected by the tremulous creeping movement of the hand, *feeling* its way in twilight, rather than firmly representing that which the artist sees.

and with the camera obscura.

The whole of this is directly the reverse of what may be effected in the camera obscura. Whether the object of an artist be to study the minutiæ of the scene before him with tasteless, insipid correctness, of which the works of Vanderheyden are the most eminent example; or whether he wishes to combine truth of representation, with tasteful selection of object, and strong delineation of character; the subject he chooses is placed completely in his power at *one view*,

view, and he has nothing to do but exert the power he has to fix it upon his paper with more rapidity, than he possibly can in any other manner.

Having now, I believe, proved that which I undertook to prove, it is time to take leave of the subject. I am sensible, that the camera lucida has many valuable properties, which it is not my object to investigate. I was induced to search for it by Dr. Wollaston's declaration, that it *possessed many advantages over the camera obscura:* I tried it, and *found it wanting*. Doubtful whether the deficiencies I found were in the instrument itself, or in my manner of using it, I sought for information by stating in your Journal the inconveniences *that I found* in attempting to use it. Mr. Bate has shown, that, by trusting to the only public information that has been given, I fell into some errours, which he has corrected; and, I think, enabled me to show, that, whatever other valuable properties it may possess, it has none that will induce an artist, who has chosen to use the camera obscura in the practice of his profession, to lay that instrument aside, and adopt the camera lucida in its stead.

I am, Sir,

Yours, &c.

No. 50, Strand,
Dec. 8, 1809.

T. SHELDRAKE.

III.

On the Acids produced by treating Ginger Root with Nitric Acid. By T. LE GAY BREWERTON, *Fellow of the Royal Physical Society, Edinburgh.*

IN No. 105 of this Journal, a new acid is announced, and proposed to be called the zingiberic. To obtain this new acid, and examine its combinations with salifiable bases, gave origin to the experiments below.

Zingiberic acid announced.

Exp. 1.

Attempt to obtain it.

Exp. 1. One ounce of Jamaica ginger root was digested five days in six ounces of nitric acid, S. Grav. 1·350, then diluted with water, kept boiling for twelve hours, saturated with carbonate of lead, the solution filtered, &c.; following, throughout the whole process, as nearly as possible the directions given by your correspondent, and ultimately a salt (*a*) was obtained "similar in appearance to short white pieces of raw silk," mixed with a little powdery matter of a yellowish white colour. Here I must observe, that the carbonate of lead which I employed was the *white lead* of the shops, and contained a considerable portion of carbonate of lime.

Salt obtained on the addition of dilute sulphuric acid.

A little of the liquor obtained after the second filtration was accidentally mixed with diluted sulphuric acid, when unexpectedly crystals (*b*) were deposited nearly similar to those obtained by evaporation, and nearly equal in quantity; although the liquor evaporated must, before evaporation, have exceeded that mixed with the diluted sulphuric acid, at least, four times.

Neither of these an acid.

The salt (*a*) and crystals (*b*), when separated from the supernatant liquor and washed, were not acid. Having failed in finding the acid in question without being able to attribute my failure to any particular cause, I determined to renew the search, altering the proportion of nitric acid employed.

Second attempt to obtain it.

Exp. 2. One ounce and a half of ginger being treated with six ounces of nitric acid in the same manner as in Exp. 1, of the liquor obtained after the second filtration four ounces were evaporated, and eight grains and a half of a salt (*c*) precisely similar in appearance to (*a*) were obtained. To another four ounces of the same liquor sulphuric acid was added, and after standing several hours a number of crystals (*d*) were deposited; which, after separating, washing, and drying at a temperature of about 200°, weighed six grains and a half, and were similar in appearance to the crystals (*b*) obtained in the first experiment. Neither the crystals (*d*) nor the salt (*c*) possessed any acidity; and from my only obtaining salts destitute of acidity, I attempted to obtain the same products without the employment of ginger.

Exp. 3.

Exp. 3. To nitric acid, as used in the above experiments, white lead was added until effervescence ceased to be produced; and to this liquor sulphuric acid was added until it ceased to throw down any precipitate; then the liquor was filtered, and sulphuric acid again added without regard to quantity. In half an hour crystals were observed depositing*, and when the deposition had ceased they were separated, and fresh additions of sulphuric acid made to the residuary liquor, so long as any crystals could be obtained. The crystallized matter (*e*) obtained in this experiment precisely resembled in appearance the crystals (*b*) in Exp. 1, and (*d*) in Exp. 2.

Attempt to obtain similar salts without ginger.

Properties of the crystallized matter obtained in the above experiments.

The products (*a*), (*b*), (*c*), (*d*), and (*e*), were of a white colour, crystals filiform, or capillary; the crystals of (*b*), (*d*), and (*e*), were longer, more slender, and more flexible than those of (*c*) and (*a*) Taste of all the crystallized products insipid.

Properties of the crystallized matter.

The crystallized matters (*a*), (*b*), and (*c*), were examined separately, and found to have the following chemical properties in common †. On exposure to a red heat they decrepitate slightly, become friable, more opake, diffusible in water, and when in a certain proportion dry quickly into a solid mass. They are sparingly soluble in water, the solution gives a dense white precipitate with barytic water, and also with oxalic acid a dense white precipitate insoluble in vinegar.

From these properties I infer, that the crystallized matter obtained in the above experiments is sulphate of lime, and must have originated from substituting white lead of the shops for carbonate of lead; however the mixture of

Sulphate of lime: but this could not have prevented the formation of zingiberic acid.

* The fact of sulphuric acid causing the deposition of sulphate of lime from a state of solution to me is not a little surprising, however it may perhaps be well known to those more versed in chemical experiments, and in no manner puzzling. B.

† As the experiments which led to the ascertaining of these properties can be of no particular utility, I have omitted inserting them. B.

carbonate of lime with carbonate of lead could not have prevented me from obtaining the acid of ginger, because zingiberic acid must have either a less, a greater, or an equal affinity with sulphuric acid for lime; if a less, the crystals of zingiberic acid ought to have been mixed with the sulphate; if a greater, the zingiberate of lime alone, or mixed with the sulphate, supposing more lime to be present than the zingiberic acid could combine with, should have been formed; and if an equal, a mixture of zingiberate and sulphate ought also to have been formed. However, to remove all doubts, experiment was again tried.

Farther attempt to obtain the acid.

Exp. 4. One ounce of Jamaica ginger coarsely powdered was infused in three ounces of nitric acid, S. G. about 1·350, at a temperature of between 45° and 55°. In a few hours the liquor assumed an olive green colour, and the ginger was reduced to a pulpy mass. During the infusion of the ginger nitrous gas was constantly disengaged, but not in any considerable quantity. At the expiration of four days six ounces of water were added, and the liquor boiled for about ten hours, water being added occasionally so as to keep the liquor about the same quantity. After boiling about five hours, the fumes disengaged ceased to become orange on mixing with the external air, but continued to smell acid, and redden litmus paper the whole time. The liquor was now of a light orange colour, much of the substance of the ginger was dissolved during the process, and the remainder was separated by filtration.

The filtered liquor neither gave cloud nor precipitate with lime or barytic water.

Crystals obtained,

(A) About two ounces of the filtered liquor were exposed to the heat of a lamp, when, after boiling briskly about five minutes, nitrous gas was disengaged, and in a little longer time the liquor became rather turbid; upon which the lamp was removed, and on cooling the liquor deposited about a drachm of crystals. These crystals were quadrilateral prisms with unequal sides, but the opposite sides equal; they were intensely acid, readily soluble in water, the solution affording with lime water a dense white precipitate insoluble in vinegar. Hence I presume these were crystals of oxalic acid.

apparently of oxalic acid.

(B) About

(B) About two ounces of the filtered liquor were exposed to a temperature below ebullition (however from inattention the liquor boiled about two minutes) until evaporated to about one sixth. This evaporated liquor on cooling was of the consistence of a sirup, and amongst it a few small quadrilateral prismatic crystals could be observed; it smelled something like burnt sugar, tasted not very acid, was readily soluble in water, the solution affording with lime water a slowly deposited white flocculent precipitate, almost completely soluble in vinegar. Hence I infer this evaporated siruplike product to be principally malic acid; and the few crystals observable, oxalic acid.

Crystals of oxalic acid with malic acid.

From the results of No. 4 I think it but fair to conclude, that the acids produced by the mutual action of nitric acid and ginger root are the malic and the oxalic; the latter, as is the case with many other vegetable substances, the result of a more complete reaction of the elements of the two agents; and this conclusion may be more readily admitted, when it is considered, that Exp. 1, 2, 3, in no manner argue against it.

No peculiar acid in ginger.

IV.

On the Method of transforming a Number from one Scale of Notation to another, and its Application to the Rule of Duodecimals. By Mr. PETER BARLOW.

To Mr. NICHOLSON.

SIR,

IT is rather the novelty, than the utility, of the rules contained in the following paper, that has induced me to send it for insertion in the Philosophical Journal, though it may possess some little of the latter; and if it will not exclude matter of more interest, it may probably be acceptable to some of your mathematical readers: in which case it is very much at your service.

On transferring numbers from one scale to another.

Yours, &c.

P. BARLOW.

Royal Military Academy, Woolwich,
Jan. 29th, 1810.

Before

Comparison of different scales of notation.

Before we enter upon the subject of transformation; it may not be amiss to make a few observations on the nature of notation in general, and the comparative advantages and disadvantages of particular systems with regard to the decimal scale of notation, which is almost universally adopted by all nations that have any knowledge of arithmetical computations.

Notation by tnes.

This singular coincidence in the division of numbers into periods of tens is a subject, that has been noticed by philosophers ever since the time of Aristotle, and is now generally attributed to the formation of man; that is to say, his having ten fingers, by the assistance of which, in all probability, calculation, or at least numbering, was first effected. See *Montucla's Histoire des Mathematiques*, vol. I.

Its origin.

Its use a late improvement.

Our present scale of notation, however, though founded on this principle, was not the immediate consequence of this division, but was an improvement introduced a long time afterward; as is evident from the works of the Greeks, who, notwithstanding they divided numbers into periods of tens, had no idea of the present system of arithmetic, the great and important advantage of which is, the giving to every digit a local, as well as a simple or natural value, and by this means being able to express any number, however large, by ten numerical characters; and, for want of which, the Greeks employed *twenty-seven*, we may say *thirty-six*, different symbols, with which they could not for a long time express a number above 10000; but they afterwards extended it to the squares of this number. See an ingenious essay on this subject, by Dalembert, at the end of the French translation of the works of Archimedes.

Greek notation.

Advantages of our method.

I have already observed, that the advantage of our present system of notation consists in giving to each character a local value, by which they increase in a tenfold proportion from the right hand towards the left; but in this I wish to be understood as merely speaking of the method, and not of the number that is selected for the radix of the system; for there is no doubt, that either 6 or 12 would have better answered this purpose, particularly the latter.

Not the best possible.

It would be extending this paper to too great a length to enter into the peculiar advantages of this or that system of notation;

notation; particularly, as the number 10, though not the best that might have been adopted, is very proper for the purpose; and as the advantages possessed by the other are not such as can lead us to expect, or even to wish, that it should ever be substituted for that, which long established custom has rendered so familiar to all our ideas of numbers. I shall therefore, in what follows, limit myself entirely to the method of transforming a number from one scale to another, and showing its application to the rule of duodecimals, which, so far as relates to the converting of a number from our own scale to any other, and its application to the rule above mentioned, I conceive to be new, and more simple than any other that I am at present acquainted with.

Method of transforming a number from one scale to another.

Let r be the radix of any system, and a, b, c, d, &c., the digits, by which any number (N) is expressed in that system, then we have

$$N = r^n a + r^{n-1} b + r^{n-2} c + r^{n-3} d, \text{ \&c.} + 9.$$

Thus 1746 in the decimal scale may be expressed by $10^3.1 + 10^2.7 + 10^1.4 + 6$, and, in the duodenary scale, 8467 may be represented by $12^3.8 + 12^2.4 + 12^1.6 + 7$. And so on for others; where it will be readily observed, that the number of characters, including the 0, in any scale of notation, will never exceed the number that expresses the radix of that system. Thus, in the binary scale only two characters are wanted, namely 1 and 0; in the senary, six; in the decimal, ten; and in the duodenary, twelve; two additional characters being necessary for expressing 10 and 11, those I shall represent thus $10 = \varphi$, and $11 = \gamma$, so that the digits of this system are as follows.

Number of characters must equal the root of the system.

$$0,\ 1,\ 2,\ 3,\ 4,\ 5,\ 6,\ 7,\ 8,\ 9,\ \varphi,\ \gamma.$$

Duodenary characters.

Now, r being the radix of any system, and a, b, c, d, &c. the digits of a number in that system; also r' the radix, and a', b', c', d', &c. the digits in any other system: then, in case of equality, we shall have $r^n a + r^{n-1} b + r^{n-2} c + r^{n-3} d$, &c. $= r'^m a' + r'^{m-1} b' + r'^{m-2} c'$, &c.; and, therefore, in converting a number from one scale to another, this equality must be established, and we must solve the above equation, the value of all the numbers being given on one side, but

on

on the second, only the value of r', the other letters representing unknown quantities: whence, as there are several unknown quantities, and only one equation, it is evident, that there are, in an algebraical point of view, various values of a', b', c, d', &c., as also of m, that will establish this equality; but the forms being limited to a certain number of digits, less than r', only one possible answer can be obtained, and this is best effected independently of any algebraical consideration.

Numbers easily transformed into the scale to which we are accustomed.

There is no difficulty in converting a number from any other scale of notation into our own, for we have only to express the numbers by means of the foregoing formula, and then collect the value of the separate terms.

Example 1.

Thus, to transform the number expressed by 74671, in the duodenary scale, into the decimal notation.

Here $74671 = 12^4.7 + 12^3.4 + 12^2.6 + 12.7 + 1$, also

$$
\begin{aligned}
\text{And } 12^4.7 &= 145152 \\
12^3.4 &= 6912 \\
12^2.6 &= 864 \\
12.7 &= 84 \\
1 &\quad\ \ 1 \\
\text{therefore } 74671 &= 153013
\end{aligned}
$$

Example 2.

Again, convert 3441 from the senary to the decimal scale.

First $3441 = 6^3.3 + 6^2.4 + 6.4 + 1$.

$$
\begin{aligned}
\text{and } 6^3.3 &= 648 \\
6^2.4 &= 144 \\
6.4 &= 24 \\
1 &= 1 \\
\text{therefore } 3441 &\quad\ \ 817
\end{aligned}
$$

Example 3.

Convert 1010110 from the binary to the decimal scale of notation.

Here $1010110 = 2^6 + 0 + 2^4 + 0 + 2^2 + 2 + 0 = 86$, so that 1010110, in the binary scale, is expressed by 86 in the common notation.

We

We have, therefore, no difficulty in transforming a number from any other scale to our own. But it is not so easy to convert a number from the decimal to another notation, though the operation is of a similar nature; but the difficulty arises in our not being so ready in the use of the other scales.

The converse more difficult.

In this case the given number is always of the form $10^n a + 10^{n-1} b + 10^{n-2} c$, &c., and the readiest way to transform it is, first to convert each of the powers of 10, beginning with the lowest, into the required scale, then each of the given terms in the formula into the same, the sum of which will be the answer. When it is only necessary to observe, that in all the operations of multiplication, and addition, we must divide by the radix of the system, setting down the overplus, and carrying the quotient, instead of dividing by 10, as is done in common arithmetic.

Rule.

As an example, let it be required to transform 1728 to the duodenary scale.

Example 4.

First $10 = \varphi$

$10^2 = \varphi \times \varphi = 84$

$10^3 = 84 \times \varphi = 6\gamma 4$

$10^4 = 6\gamma 4 \times \varphi = 5954$

&c.

Also $1728 = 10^3 + 10^2.7 + 10.2 + 8.$

$8 = 8 = 8$

$10.2 = \varphi \times 2 = 18$

$10^2.7 = 84 \times 7 = 4\varphi 4$

$10^3.1 = 6\gamma 4 \times 1 = 6\gamma 4$

therefore $1728 =$ 1000 *Ans.*

This example being understood, the transformation in other cases will become very easy: thus in the following examples, transform 71671 to the duodenary scale.

First, $71671 = 10^4.7 + 10^3.1 + 10^2.6 + 10.7 + 1.$

Example 5.

Now

$$
\begin{array}{lrcrcr}
\text{Now} & 1 = & & 1 & = & 1 \\
 & 10.7 = & & \varphi \times 7 & = & 5\varphi \\
 & 10^2.6 = & & 84 \times 6 & = & 420 \\
 & 10^3.1 = & & 6\gamma 4 \times 1 & = & 6\gamma 4 \\
 & 10^4.7 = & & 5954 \times 7 & = & 34614 \\
\hline
 & & & & & 35587 \ \textit{Ans.}
\end{array}
$$

Example 6. Transform 11111 to the senary scale.

Here $11111 = 10^4 + 10^3 + 10^2 + 10 + 1$.

$$
\begin{array}{lclcr}
1 & = & 1 & = & 1 \\
10 & = & 14 & = & 14 \\
10^2 & = & 14 \times 14 & = & 244 \\
10^3 & = & 244 \times 14 & = & 4344 \\
10^4 & = & 4344 \times 14 & = & 114144 \\
\hline
 & & & & 123235 \ \textit{Ans.}
\end{array}
$$

Example 7. Convert 1248 into the binary scale.

First $1248 = 10^3 + 10^2.2 + 10.4 + 8$.

$$
\begin{array}{lclcr}
10 & = & 1010 & & \\
10^2 & = & 1010 \times 1010 & = & 1100100 \\
10^3 & = & 1100100 \times 1010 & = & 1111101000 \\
\text{again } 8 & = & 1000 & = & 1000 \\
10.4 & = & 1010 \times 100 & = & 101000 \\
10^2 \times 2 & = & 1100100 \times 10 & = & 11001000 \\
10^3 & = & & & 1111101000 \\
\hline
 & & & \textit{Ans.} \ldots\ldots & 10011100000
\end{array}
$$

Easy in practice. Various other examples might be given, but the above are sufficient for pointing out the method of transformation, which will become remarkably easy with a very little practice. We might also pursue the subject still farther, by converting a number from one scale to another, neither of which is the decimal; but this is perhaps better effected, by first transforming the numbers into the decimal scale, and afterwards into that proposed.

Application. I shall conclude now with showing the application of what has

has been said to the rule of duodecimals, or cross multiplication.

to the rule of duodecimals, or cross multiplication.

To multiply feet, inches, parts, &c. by similar denominations.

Rule. Transform the number of feet, if above 12, into the duodenary scale, and set the inches, parts, &c. as decimals; then multiply as in common arithmetic, except, carrying for every 12, instead of every 10, as in common operations.

Example. Multiply 3 feet, 5 inches, 7 parts, by 3 feet, 11 inches, 2 parts.

```
Here    3·57
        3·γ2
        ----
         6γ2
       3215
       γ49
      -------
      12·8542 =14 feet, 8′ 5″, 4‴,  2⁗.
```

Multiply 17 feet, 3′, 4″ by 19 feet, 5′, 11″.

```
        15·34
        17·5γ
        ------
        13φ08
        7248
       φ0γ4
      1534
      --------
      240·9688 =336 ft. 9′, 6″, 8‴, 8⁗.
```

Find the solidity of a cube, the side of which is 3 feet, 7 in. 7 parts.

```
        3·77
        3·77
        ----
        2151
       2151
       φφ9
      -------
      11·2361
          377
      -------
       784067
      784067
     336φ63
     ----------
     3γ·φφφφ17 =47 ft. 10′, 10″, 10‴, 10⁗, 1, 7.
```

And

This preferable to the common rule.

And thus may any operation in duodecimals be performed as readily as common multiplication, and the result obtained to the greatest degree of accuracy, on both of which accounts, I consider is as preferable to the rule commonly given for this purpose.

V.

On the Propagation of Sound through unelastic Fluids. In a Letter from FRANCIS ELLIS, *Esq.*

To Mr. NICHOLSON.

SIR,

Propagation of sound through water.

I Have accidentally become acquainted with a fact relative to the propagation of sound through unelastic fluids, which seems new; and if really so, you may perhaps deem it of sufficient interest, to be made known through the medium of your Journal.

The sound occasioned by the condensation of steam.

Bathing in a warm bath at Cheltenham, and plunging my head under the water, I was suprised by a harsh, disagreeable sound, and jar through both ears, resembling the sound and sensation, that would, I imagine, be received, were a sheet of tin laid on each ear, and struck simultaneously with two small hammers. Induced by curiosity to examine whence this could proceed, I observed, that the bath, which is ten or twelve feet square, was heated by steam conveyed into it at one of its angles by a metallic tube; and that the condensation of the steam produced at intervals in this tube a dull clicking sound. By immersing one ear in the bath, and keeping the other in the air, I ascertained, that it was this sound altered and augmented by being propagated through the water of the bath, which had caused the harsh snapping noise, and jarring sensations, that I have already described. The distance of my head from the entrance of the steam-tube into the bath was seven or eight feet: and it is remarkable, that, when it was submerged, I had distinct, though simultaneous sensations of the jarring sound in each ear.

I am, Sir,

Your very obdient servant,

FRANCIS ELLIS.

Somerset Place, Bath,
10th Dec. 1809.

Observ-

OBSERVATION.

This noise is of the same nature as that of simmering; (See Philos. Journal, vol. XI, p. 216) and, like that it is produced by the collapsion of the water rushing into the vacuum left by the bubbles of steam. It does not seem easy to account for the distinct sensations in the two ears. I incline to think, that it may have arisen from the direction of the undulatory motion having been more favourable to one ear than to the other according to position, and that the ear most directly opposed to the undulation may have received a stronger action than the other. If this were the case, the sensation ought to be single when the face was directly opposed to the steam orifice.

Of the same nature as that of simmering.

W. N.

VI.

Miscellaneous Observations on some Phenomena of Combustion. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

NOT knowing whether your publication takes in such trifling observations as what I am about to offer, I leave it to your choice to insert them or not; but not having met with them in my own reading, I conceive they may be acceptable to some of your younger readers.

1. I have made at different times phosphorus bottles, to serve instead of a flint and steel (by simply melting the phosphorus in a dry and heated phial, and allowing it to burn for a few seconds); and I find, that, though kept with a well ground stopper, they always get wet after using about twelve or fifteen times, probably from the phosphoric acid attracting water from the atmosphere at the times of opening the bottle. This, beside the expense attendant on renewing them, sometimes proves a mortifying disappointment;

Phosphorus bottles.

ment; but by dipping the match as usual, and then rubbing it briskly upon a piece of old woollen cloth, or even brown paper, it will almo t certainly inflame.

Enlarged flame of an Argand lamp.

2. When the leaking vessel of an Argand lamp is removed, after it has filled with oil, the lamp continuing lighted, the flame rises to the height of several inches. Long after I had seen this I was unable to account for it, but it arises no doubt from the gas being confined in the inner cylinder, which will not inflame till the admission of a current of air, for the flame does not rise unless the receiver has been full for a short time, nor does it ever rise above a certain height.

Candle rekindled without contact of flame.

3. When I first began to attend to Natural Philosophy, an eminent lecturer, a chemist, whose course I attended, gave as an instance of attraction, "A candle just blown out is relighted on the approach of a lighted body, as it attracts the flame without touch:" and it was long before I saw the absurdity of this explanation, by having recourse to the abstruse doctrine of attraction, instead of saying, the carburetted hidrogen, that is produced, inflames, and thereby creates a fresh supply of inflammable gasses.

G. O.

Camphor in carraway seed.

I find that camphor is contained in considerable proportion in the seeds of carraway *(carum carui)*. 1 lb. of seed yields about 4 oz. of oil; and ¼ oz. of camphor.

REPLY.

Remarks.

Many of the greatest discoveries have arisen from circumstances apparently trifling; for which reason they ought never to be considered as unworthy of attention. The first observation of G. O. is known to the venders of phosphorus apparatus, who generally put a piece of cork for rubbing the match upon.—I am disposed to think, that the augmentation of the flame in an Argand lamp, when the oil cup is taken off, arises merely from the increased passage for the ascending air. If the hand or any other flat surface be held beneath the lower orifice after the oil cup (as usually made) has been removed, all the variations from the brilliant

brilliant white to the brown smoaky flame may be observed with great advantage, accordingly as the hand is held more or less remote from the aperture.

W. N.

VII.

On the Process for procuring the Metal of Potassium by Means of Iron. In a Letter from a Correspondent.

SIR,

WISHING to repeat some of the brilliant experiments of Mr. Davy, I endeavoured to procure the metal of potash by means of iron, in which I have repeatedly failed. As I perceive Mr. Curaudau, a French chemist, has also failed in procuring potassium in this way, I have taken the liberty to request you will communicate in your next number every information you may be possessed of with respect to the minutiæ of the process, and the best and readiest method of constructing an apparatus. I hope you will attend to this, being persuaded, that it is the difficulties that are first met with which have prevented other chemists from following the steps of Mr. Davy.

Query on the method of procuring potassium by means of iron.

I remain, Sir,

Your obedient reader,

A. B.

P. S. I understand Mr. Hachett has written something on the subject, but I have not seen an account of his experiments.

REPLY.

As the experiment never fails in the laboratory of the Royal Institution, I apprehend a brief account of the manner in which it is there performed will be the most satisfactory answer I can give to my correspondent.

A common gunbarrel, made very clean, and bent in the manner

Method described.

manner represented at fig. 6, plate IV, has an iron receptacle, A, ground into one end of it, and furnished with a ground stopper. This receptacle contains from two to three ounces of potash (pure potash) that has been in fusion. Very clean iron turnings are placed in the middle of the barrel D, which is heated to whiteness; and the potash is kept cool till this temperature is attained. The potash is then slowly fused, and it passes through a hole in the receptacle upon the turnings. The heat is kept up till gaseous matter ceases to come over. The extremity of the tube B is kept cool, and in this the potassium collects. Common air is prevented from entering by a glass tube, C, which supports the weight of a column of mercury.

VIII.

Description of a reflective Goniometer. By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S**.

Crystallography much advanced.

FROM the advances that have been made of late years in crystallography, a very large proportion of mineral substances may now be recognised, if we can ascertain the angular dimensions of their external forms, or the relative position of those surfaces, that are exposed by fracture. But though the modifications of tetrahedrons, of cubes, and of those other regular solids, to which the adventitious aid of geometry could be correctly applied, have been determined with the utmost precision, yet it has been often a subject of regret, that our instruments for measuring the angles of crystals are not possessed of equal accuracy, and that in applying the goniometer to small crystals, where the radius in contact with the surface is necessarily very short, the measures, even when taken with a steady hand, will often deviate too much from the truth to aid us in determining the species to which a substance belongs.

But a good instrument for measuring the angles of crystals still wanting.

A means of remedying this defect has lately occurred to me, by which in most cases the inclination of surfaces may

This defect supplied.

* Philos Trans. for 1809, p. 253.

be

be measured as exactly as is wanted for common purposes, and when the surfaces are sufficiently smooth to reflect a distinct image of distant objects, the position of faces only $\frac{1}{50}$ of an inch in breadth may be determined with as much precision as those of any larger crystals.

By using a lengthened radius.

For this purpose, the ray of light reflected from the surface is employed as radius, instead of the surface itself, and accordingly for a radius of $\frac{1}{50}$ of an inch, we may substitute either the distance of the eye from the crystal, which would naturally be about twelve or fifteen inches; or for greater accuracy we may, by a second mode, substitute the distance of objects seen at a hundred or more yards from us.

Instrument described.

The instrument which I use consists of a circle graduated on its edge, and mounted on a horizontal axle, supported by an upright pillar (Plate V.) This axle, being perforated, admits the passage of a smaller axle through it, to which any crystal of moderate size may be attached by a piece of wax, with its edge, or intersection of the surfaces, horizontal and parallel to the axis of motion.

Method of using it.

This position of the crystal is first adjusted, so that by turning the smaller axle, each of the two surfaces, whose inclination is to be measured, will reflect the same light to the eye.

The circle is then set to *zero*, or 180°, by an index attached to the pillar that supports it.

The small axle is then turned till the farther surface reflects the light of a candle, or other definite object to the eye; and, lastly, (the eye being kept steadily in the same place) the circle is turned by its larger axle, till the second surface reflects the same light. This second surface is thus ascertained to be in the same position as the former surface had been. The angle through which the circle has moved is in fact the supplement to the inclination of the surfaces; but as the graduations on its margin are numbered accordingly in an inverted order, the angle is correctly shown by the index, without need of any computation.

Angle of an irregular fracture may be measured by it.

It may here be observed, that it is by no means necessary to have a clean uniform fracture for this application of the instrument to the structure of laminated substances; for since all those small portions of a shattered surface, that

are

are parallel to one another (though not in the same plane), glisten at once with the same light, the angle of an irregular fracture may be determined nearly as well, as when the reflecting fragments are actually in the same plane.

Small errour from parallax.

In this method of taking the measure of an angle, when the eye and candle are only ten or twelve inches distant, a small errour may arise from parallax, if the intersection of the planes or edge of the crystal be not accurately in a line with the axis of motion *; but such an errour may be rendered insensible, even in that mode of using the instrument, by due care in placing the crystal; and when the surfaces are sufficiently smooth to reflect a distinct image of objects, all errour from the same source may be entirely obviated by another method of using it.

May be obviated.

For this purpose, if the eye be brought within about an inch of the reflecting surface, the reflected image of some distant chimney may be seen inverted beneath its true place, and by turning the small axle may be brought to correspond apparently with the bottom of the house (or with some other distant horizontal line.) In this position the surface accurately bisects the angle, which the height of that house subtends at the eye (or rather at the reflecting surface); then, by turning the whole circle and crystal together, the other surface, however small, may be brought exactly into the same position; and the angle of the surfaces may thus be measured, with a degree of precision which has not hitherto been expected in goniometry.

Accuracy of the instrument.

The accuracy, indeed, of this instrument is such, that a circle of moderate dimensions, with a vernier adapted to it, will probably afford corrections to many former observations. I have already remarked one instance of a mistake that prevails respecting the common carbonate of lime, and I am induced to mention it, because this substance is very likely

* I cannot omit mentioning, that Mr. Sowerby had thought of employing reflection for this purpose, nearly at the same time as myself; but did not succeed to his satisfaction, in consequence of an attempt to fix the position of the eye. For when the line of sight is determined by a point connected with the apparatus, the radius employed is thereby limited to the extent of the instrument, and the errour from parallax is manifestly increased.

to

to be employed as a test of the correctness of such a goniometer, by any one who is not convinced of its accuracy from a distinct conception of the principles of its construction.

The inclination of the surfaces of a primitive crystal of carbonate of lime is stated, with great appearance of precision, to be 104° 28′ 40″: a result deduced from the supposed position of its axis at an angle of 45° with each of the surfaces, and from other seducing circumstances of apparent harmony by simple ratios. But however strong the presumption might be that this angle, which by measurement approaches to 45°, is actually so, it must nevertheless be in fact about 45° 20′; for I find the inclination of the surfaces to each other is very nearly, if not accurately 105°, as it was formerly determined to be by Huygens*; and since the measure of the superficial angle given by Sir Isaac Newton† corresponds with this determination of Huygens, his evidence may be considered as a further confirmation of the same result; for it may be presumed, that he would not adopt the measures of others, without a careful examination.

Mistake in the angles of carbonate of lime corrected by it,

and made to agree with the measures of Huygens and Newton.

In the annexed plate,

a [illegible] the principal circle of the goniometer graduated on its edge.

Explanation of the plate.

c c. The axle of the circle.

d. A milled head by which the circle is turned.

e e. The small axle for turning the crystal, without moving the circle.

f. A milled head on the small axle.

g. A brass plate supported by the pillar, and graduated as a vernier to every five minutes.

h. The extremity of a small spring, by which the circle is stopped at 180°, without the trouble of reading off.

i i and *k k*. Are two centres of motion, the one horizontal, the other vertical, for adjusting the position of a crystal: one turned by the handle *l*, the other by the milled head *m*.

The crystal being attached to a screw-head at the point *n*

* Huygenii Opera Reliqua, Tom. I, p. 73—Tract. de Lumine.

† Newton's Optics, 8vo, p. 339, Qu. 25, concerning Iceland Crystal.

(in the centre of all the motions), with one of its surfaces as nearly parallel as may be to the milled head *m*, is next rendered truly parallel to the axis by turning the handle *l* till the reflected image of a horizontal line is seen to be horizontal.

By means of the milled head *f*, the second surface is then brought into the position of the first, and if the reflected image from this surface is found not to be horizontal, it is rendered so by turning the milled head *m*, and since this motion is parallel to the first surface, it does not derange the preceding adjustment.

IX.

Remarks upon Meteorology: with a Specimen of a new Meteorological Table. By J. BOSTOCK, *M.D.*

Predictive meteorology of great use.

THAT part of the science of meteorology, which consists in predicting the changes of the weather, is not only interesting from its connection with the great operations of nature, but is also in itself of obvious practical utility. It has accordingly engaged the attention of many philosophers; and numerous attempts, some derived from hypothesis, and others founded upon supposed experience, have been formed, to lay down a set of rules, which might apply to the various circumstances that present themselves. The basis of these rules must be observation; and it is from a conviction of this truth, that many scientific men, in different parts of the world, have assiduously employed themselves in forming what are called meteorological tables. But, I think, I may venture to assert, that the benefit derived from these tables of observations by no means corresponds with the trouble that has been bestowed upon them. After marking down the height of the barometer and thermometer, noting the direction and force of the wind, and measuring the quantity of rain, for years together, it does not appear, that the observer is in any degree enabled to determine, whether the next day will be fair or foul, calm or stormy. On the con-

It must be founded on observation.

The benefit arising from meteorological tables not yet answerable to their trouble.

trary

trary we know, that there are many persons who are ignorant of all the principles of science, and have never systematically studied the subject, who have notwithstanding arrived at that practical knowledge, which gives the study its principal value. I now refer to farmers, sailors, and such other persons as are led by their occupations to spend much of their time in the open air; and to whom a knowledge of the changes of the weather is of immediate importance. Every one who has conversed much with these persons must know, that they have acquired great sagacity in predicting these changes; and yet it unfortunately happens, that their knowledge is of little use to any one but themselves, not being in the habit of conversing upon the subject, and having acquired their ideas solely by the exercise of their own faculties, they are often absolutely unable to convey to any one else the reasons by which their own judgment has been determined. It is now some years since I became impressed with this view of the subject, and since I resolved to endeavour to imitate the method by which this practical knowledge appears to be obtained, and at the same time to frame a nomenclature, by which I might be able to convey my ideas to others. Although my mode of life, as a resident in the middle of a large town, has been unfavourable to such pursuits, yet I think that I have been not altogether unsuccessful in my attempt; some points of importance I consider myself as having ascertained, and many more have suggested themselves, which a longer series of observations must confirm or refute.

More has been done without the advantage of science.

But this knowledge is confined to the individual.

These two modes of attaining knowledge might be combined.

My plan of observation, as it appears, essentially consists, first, in paying a constant attention to atmospherical phenomena of all kinds; and secondly, in adopting a nomenclature, by which these phenomena may be accurately recorded, and by which the observations made at different times, and in different places, may be compared together. In the meteorological journals which have been hitherto published, either by learned societies, or by individuals, we have had the height of the barometer and thermometer taken at two or three stated periods in the course of the day, the degree of the hygrometer is frequently added, the quantity of rain that has fallen, the force and direction of the wind, and

Attempt to effect this.

Usual meteorological tables.

and occasionally there are subjoined to these a few very general remarks as to the cloudiness or brightness of the atmosphere, the presence of fogs, mists, &c.* It is no doubt necessary and desirable to ascertain these points; but they are very far from being the most important objects of attention. What I have been in the habit of considering as such are first the nature and shape of the clouds, and the progressive changes which they are undergoing; secondly, the relative state of the barométer, whether rising or falling, and with what degree of rapidity; and thirdly, the relative state of the wind, whether increasing or diminishing, whether it has lately changed its direction, and from what point it proceeded before its change. These are the objects to which the attention should be uniformly directed, and which may be considered as the basis of all meteorological predictions. There are also a number of accessary circumstances, that may be occasionally employed. Of these some of the most important are, any sudden or remarkable changes in the temperature or humidity of the atmosphere; the aspect of mountains or other distant objects; the appearance of the horizon as contrasted with that of the higher regions of the atmosphere; the state of the air as to fogs, mists, dew, haze, &c.; the electric state of the air; the manner in which smoke is affected in passing from chimney tops; and the state of evaporation from the surface of the earth. It will be evident, that to enter into a full description of all the modifications and varieties of these different phenomena would require a long dissertation; my present object is however merely to offer a specimen of my diary, and to accompany it with such explanations as may render it intelligible, and at the same time illustrate its principles.

Most important objects in meteorology.

Other circumstances to be noticed.

I have selected as a specimen of my diary that part of it, which contains an account of the weather in September last, as it was marked by many striking changes, which will give me an opportunity of explaining several different parts of my

Specimen of a diary.

* See the Journal published by the Royal Society, that in the Journal de Physique, that in the Bibliotheque Britannique, Bent's Meteorological Tables, the table published in this work, in the Athenæum, and in the Philosophical, Monthly, and Gentleman's Magazines.

my system. I must observe, that neither this, nor any other part of my diary, is in what I consider as a perfect state; my observations were necessarily interrupted from a variety of causes, and it is impossible, that any one not residing in the country, and being much in the open air, could pay that constant attention to atmospherical phenomena, which I regard as so important. I may premise, that the preceding month of August, although wet, was remarkable rather for the frequency, than the long continuance of the rains; it was a bad harvest month, but might have passed for seasonable spring weather; the temperature was low, but uniform. Towards the end of the month there were some warmer and drier days, and the farmers began to hope that a fine autumn was commencing; the event however proved extremely contrary to their expectations.

Weather of August 1809.

September, 1809.

1 SE 2 — 29·72— Partially clear, some light clouds, nearly calm. | Continued all the day nearly clear and calm with a gentle

SE 2 — 29·65— breeze. | Evening clear and calm; a very pleasant day. Mountains clear, but light. No tufts or lines, but a few small, round, dark clouds; afternoon transparent.

2 SE 3 — 29·60— Uniformly cloudy, gentle breeze. | Afterwards some breaks, the breeze increased. At noon some rain came on for 2 hours. Afternoon cleared partially, and wind went to E. Then some

E 6 — 29·50— heavy broken clouds, and an imperfect arc between E and W. | Evening wind high, partially clear, driving showers in the night.

3 E 5 — 29·48 Fresh breeze, heavy clouds, tendency to rain. | Forenoon more clear and less windy. Afternoon a heavy shower, then partially clear and a gentle breeze. |

E 3 — 29·50+ Evening some lightning. The state of the atmosphere, which produced the

squall

			squall of the 2d, now changed, but wind still E.
4	E 4	29·59	Partially clear, breeze, dense clouds. | Forenoon partially clear, warm, pleasant, gentle breeze. Then became cloudy and continued so the rest of the
	E 2	29·53	day, nearly calm. Tendency to rain in the afternoon. In the forenoon a precipitating arc S to N, in the afternoon went to SE. Small contiguous solar halo; clouds moved rapidly from SE. | Evening dark, calm, uniformly cloudy.
5	NE 1	29·45—	Uniformly cloudy and calm. | Continued all the day uniformly cloudy, except a short time about noon. Almost quite calm, wind went quite round to W, S, and again to NE. Afternoon
		29·41	rather foggy. Evening dark and calm.
6		29·41—	Rain in the night, cloudy, calm. | Grew partially clear about noon, and at 3 P. M. a gentle breeze sprung up from NNW. Pleasant, but close day.
	NNW 2	29·28—	Evening a good deal of lightning and some heavy showers.
7	ENE 1	29·23—	Cloudy with some breaks, nearly calm. Forenoon grew partially clear, and
		29·16	breeze increased. At noon heavy showers; then partially clear, gentle breeze,
	E 2	29·20+	lightning in the afternoon. | Evening bright, almost perfectly clear, gentle breeze. The barometer, which had been falling almost uniformly for the last seven days, began to rise about 6 P. M.
8	E 1	29·30+	Partially clear, calm. | Continued calm during the greatest part of the day; a gentle breeze from NNW in the afternoon. In the forenoon there was a precipitating arc from ESE. Gradually grew

Day	Wind	Barometer	Remarks
			grew more clear, and in the afternoon nearly so; dissolving constitution; plea-
	NNW 2	29·38+	sant day. | Evening bright. Lightning in the night.
9	W 4	29·49+	Cloudy, thick body of clouds moving before the wind; breeze. | A tendency to clear early in the forenoon, but again became cloudy, and about noon a little
	NW 5	29·62+	rain. Afternoon round shaded body of clouds moving before the wind. Cold, irregular, fresh breeze. Large piled clouds at sun-set. Sour day.
10	S 3	29·56—	Bright, nearly clear, gentle breeze. Some lines from W to E. | Large rolling clouds began to form about 9, gradually grew more cloudy; at 12 it began to rain and rained for 2 or 3 hours.
	W 1	29·55	About 3 P. M. wind went to W, at first a breeze, then grew more calm. Became more clear. | Evening very clear and calm. In the afternoon an imperfect wreathed arc from NW.
11	W 4	29·58+	Cloudy with breaks, breeze. | Continued so during the day, generally bright with large clouds, sometimes rounded and rolling, sometimes heavy and bro-
	W 3	29·66+	ken. Cold breeze. | Evening cloudy, breeze.
12	W 4	29·68+	Bright, partially clear, cool breeze. Clouds rather broken, moving before the wind. | Continued all the day bright, some heavy clouds, breeze, inclined to
	WSW 2	29·70	squally. | Evening bright, clear, with some heavy clouds; nearly calm. At sunset wind went more towards S. This morning seemed like a clearing day, but at sunset the mountains were transparent.
13	SSE 2	29·69—	Partially clear, gentle breeze. | Clouds increased in the forenoon, and at noon

uniformly

Day	Wind	Barom.	Remarks
			uniformly cloudy. Heavy and gray atmosphere, shaded towards the horizon. About 3 began to rain, and continued
	SSE 5	29·56—	more or less all the afternoon and evening. Cold and fresh breeze.
14	SE 3	29·52	Partially clear, gentle breeze. | In the forenoon irregular tufts, generally pointing to W. Shower about noon. At 1 wind went to E, clouds dense
	ENE	29·74+	and broken, now more clear. | Evening very clear, starlight, gentle breeze; in the afternoon irregular tufts tending to N.
15	NE	29·92+	Partially clear, calm, veering. | In the forenoon dense clouds. In the afternoon an arc from NNW, with some flocks pointing to E. Gentle breeze
	NNW 2	30·02	from NNW. | Evening cloudy, nearly calm.
16	S	29·92—	Uniformly cloudy, nearly calm. | Misty rain came on at 8, and continued for 2 hours. About 10 wind went to W, and became a fresh and squally breeze; tendency to clear at noon. Mountains black and large; a low stratum of clouds moving rapidly from SW. In
	W 5.3	29·85	the afternoon thick rain, squally breeze. Evening partially clear and more calm.
17	W	29·85	Large heavy clouds slowly moving before the wind. | Showers during the day, clouds with some breaks, breeze.
	W 2	29·85	In the afternoon cleared, and the evening was bright and nearly calm.
18	SSE 4	29·65	Seemed to have rained much in the night, now heavy rain, uniformly cloudy, nearly calm, clouds moving rather rapidly from SE. | Rain continued until noon, then showers. Wind went to W, barometer continued falling rapidly. Partially clear, large clouds floating on

Day	Wind	Barometer	Remarks
			on a gray ground. Wind squally in
	W 4.6.7	29.26+	the afternoon; about 8 became high, and at 10½ grew a violent storm; barometer began to rise about 10.
19	WNW	29·56+	Some squally showers in the night, still windy, partially clear, clouds moving from NW. | In the forenoon heavy showers. About noon became more
	WNW	29·63+	clear and calm; afternoon bright, gentle breeze. | Evening nearly calm, at first nearly clear, then more cloudy.
20	SE	29·27—	Heavy rain, uniformly cloudy, nearly calm. | In the forenoon wind went to W, breeze, partially clear, with occasional heavy showers. | In the evening
	W •	29·09+	the wind went to W, became high with squally showers. Barometer had fallen ·54 in 24 hours, now tends to rise.
21	W	29·53+	Still high wind; bright, partially clear, large dense, shaded clouds, moving before the wind. | Continued bright with large clouds, breeze. Barometer had risen ·44 in 12 hours, but about noon
	SSW 3	29·58—	tended to fall. Afternoon lines pointing to W; then grew uniformly cloudy and precipitating; gentle breeze.
22	S 3	29·33+	Rain in the night, and wind. Now rain, uniformly dull, breeze. | About 10 cleared partially with a breeze; at
	SW 3	29·43	noon went to SW; during the rest of the day sometimes bright, sometimes considerably clouded; precipitating.
	S 3	29·40—	Wind returned to S in the evening. About noon there were many irregular tufts, the ends pointing to SE; when the wind went to SW the tufts all disappeared, and white rolling clouds formed. In the afternoon an imperfect arc from SW.

Rain

23	WSW 3	29·92+	Rain in the night; partially clear, clouds slowly moving from SW. | Continued so until noon, then it grew more cloudy, and then a heavy shower. Wind
	W 6·3	29·55+	went to W and was high for some hours. A heavy shower between 4 and 5 P.M. Then became more clear and calm, large clouds moving before the wind; gentle breeze. Lightning.
24		29·64+	Very heavy showers in the morning, calm. | For some time partially clear;
		29·67	then heavy rain for a considerable part of the forenoon. Large shaded clouds,
	SW 3	29·64—	calm. Afternoon gentle breeze, pleasant. | Evening more cloudy with showers, nearly calm. Lightning.
25	W 3	29·56+	Rain in the night, still showery; partially clear, broken clouds, breeze. |
	WNW 3	29·73+	In the afternoon some heavy rain, breeze. In the forenoon an imperfect precipitating arc from SW, with rolling clouds moving before the wind; mountains clear, not dark. | Evening bright; round clouds slowly moving before the wind, dissolving; nearly calm.
26	W 3	29·81	Rain in the night; partially clear, clouds heavy and broken, moving slowly before the wind. | During the day irregular
	SW 4	29·50—	breeze, pleasant. About noon long, irregular wreathed arc from NNW to SSE, some crossing lines pointing to SW. Afternoon more cloudy, precipitating sky, a little rain, irregular breeze.
27	W 4·6	29·16—	Rain in the night, which still continues; uniformly cloudy with breeze. | Forenoon wind rose and for some hours was very high, with violent showers of rain
	NNW 4	29·55+	and hail. About 4 wind abated, went to N, atmosphere cleared, and became dissolving.

			dissolving. | Evening cool, fresh breeze from NNW; bright with some dense clouds.
28	NW 6	29·68+	Windy in the night, still continues so; partially clear, dense and broken clouds moving before the wind. | During the
	NW 5	29·83+	day cold fresh breeze; partially clear, large shaded clouds, some flying showers. | Evening more calm and clear.
29	NNW 3	29·85	Partially clear, dense clouds moving slowly before the wind. | During the day sometimes bright, partially clear;
	W 2 S 1	29·82—	Forenoon feathered arc from N; lower stratum of clouds moving slowly from W; muddy with dark clouds in N. | Evening cloudy, calm. Reverse resolving day.
30	S 2 W 4	29·70+	Uniformly cloudy, nearly calm. | Mild, misty rain for 2 or 3 hours in the forenoon; about noon wind went to W, fresh breeze sprung up and it cleared; wind then went to NW, grew bright,
	NNW 2	29·89+	clear, and nearly calm. | Evening brilliant, a few clouds, nearly calm, dissolving.

The diary consists of two columns, the first for the direction and force of the wind, the second for the height of the barometer, and to these is appended a large space for occasional remarks. For the direction of the wind I think it sufficiently minute to divide the compass into 16 points; N, NNE, NE, ENE, E, &c. for the other quarters. The force of the wind is comprehended under 8 general terms, which have each their appropriate figure. 1, calm; 2, nearly calm; 3, gentle breeze; 4, breeze; 5, fresh breeze; 6, windy; 7, high wind; 8, violent storm. There are some other circumstances connected with the wind, whether the force be uniform, or whether it be irregular, what is usually called squally; whether it be increasing or diminishing at the time of the observation; whether it blow steadily

Diary explained.

Wind.

steadily from one point, or veer about; whether it shift irregularly or revolve gradually; and lastly, whether it appear to extend high into the atmosphere, or only affect the lower stratum.

Barometer.

The height of the barometer is noticed between 8 and 9 in the morning, and at the same hour in the evening; but its absolute height is of little consequence, unless we know whether it be rising or falling, when it began to rise or fall, and also what connection there is between these circumstances and the direction of the wind, and the shape and appearance of the clouds. The following case will illustrate the greater importance of *relative* than of *absolute* observations upon the subject. Suppose that nothing was known of the state of the preceding day, but in the morning we find the barometer at 29·75, its medium height, the wind W, a gentle breeze, and the sky partially clear. These circumstances afford no decisive indication of the probable state of the day, and according to the preceding appearances, they may afford either a favourable or an unfavourable prognostic. If the evening before the wind had been NW, the atmosphere brilliant, and the barometer 29·80, we may expect, that the wind is travelling to the SW, and that rain will probably ensue; but, on the contrary, if the preceding day had been rainy, the wind in a southerly point, and the barometer low, we may fairly hope that an improvement will take place in the weather. It is in my opinion principally for want of this plan of retrospective observation, that our meteorological instruments are of so little use.

Clouds.

The occasional remarks form by far the most important part of the above table, and upon them I shall now proceed to make some comments. The phenomena of the clouds I consider as forming the most important part of my system of observation, and they require a great variety of terms to designate all their characters. They are varied as to their general shape, size, colour, density, height from the surface of the Earth, and motion. Beside these there are three points particularly to be attended to, whether their bulk be increasing or diminishing; whether any change be taking place in their character, and one kind of cloud be gradually

dually passing, or becoming converted into another species; and what is their relation with respect to the state and direction of the wind. To enter into a full detail of this subject would alone occupy a long dissertation, I shall merely take occasion to illustrate some parts of it in the explanation which I shall give of the specimen of my journal. In the account of the state of the weather on the 1st of September I have mentioned the appearance of distant objects. Those to which I refer are the mountains, which are seen in different directions from the vicinity of Liverpool. To the W and SW we have a long line of hills situate in the counties of Flint and Denbigh, from 20 to 30 miles distant, and beyond these the summits of some of the Caernarvonshire mountains are visible. To the S we have the high lands in the SW of Cheshire, and beyond these are occasionally seen the peaks of the Montgomeryshire mountains. To the E and SE is the range of hills that separates the counties of York and Lancaster, and also those in the N of Derbyshire and Staffordshire. It is not in many situations, that the spectator has an opportunity of seeing all these objects from one point of view, nor is it often that the state of the atmosphere renders them all visible at any one time; but it will be perceived, that our situation is very favourable for observations of this kind. Distant objects vary as to their apparent size, their distinctness, and their colour.

Appearance of distant objects.

Under the title of particular states of the atmosphere I mean to designate some phenomena, which are sufficiently obvious and characteristic, but the immediate cause of which is doubtful. I have observed in my notes, that the afternoon of the 1st of September was transparent, and I shall endeavour to describe what I mean to designate by this term. A transparent condition of the atmosphere seldom lasts for more than a few hours, and these generally before sunset. The sky is either clear, or if there be any clouds, they consist of fine lines lying parallel to each other, or of irregular tufts in the upper parts of the sky, or of small, round, spotted clouds near the horizon. What however gives the character and name to this kind of day is the peculiarly beautiful appearance, which moderately distant objects

State of the atmosphere.

objects present, especially any large expanse of water. I cannot describe it better than by saying it is like a picture very highly varnished. Another characteristic circumstance in a transparent atmosphere is the appearance of smoke as it ascends from a chimney; it mounts up for a considerable height in nearly a straight line, and in a slender column, and is a much longer time than usual in diffusing itself through the air. The sun frequently sets with a degree of mist, but often surrounded by the most brilliant colours, and for some time after its orb has disappeared, the whole horizon exhibits an extremely delicate lilac or violet tinge.

Sign of rain.

Every one who has not been in the habit of minutely attending to the states of the atmosphere is ready to exclaim, that the weather has every appearance of being settled, and that we are at least certain of the following day being fine. My observations however lead me to the conclusion, that there is no more infallible sign of a change of weather, than one of these transparent evenings.

Liverpool, Feb. 5, 1810.

(To be continued.)

X.

On the Nature of the intervertebral Substance in Fish and Quadrupeds. By EVERARD HOME, *Esq. F. R. S.**

Peculiarity in the spine of the shark.

IN examining the internal structure of a squalus maximus of Linnæus, that lately came under my observation, a description of which will be the subject of a future paper, I met with a peculiarity in the intervertebral substance of the spine not hitherto made known to the public.

Fluid in the intervertebral substance.

The fish is thirty feet six inches long, the diameter of the larger vertebræ near the head, seven inches. The intervertebral substance was cut into by Mr. Clift four days after the fish was brought on shore, and a limpid fluid rushed out

* Philos. Trans. for 1809, p. 177.

out with so much velocity, that it rose to the height of four feet.

At the end of twelve days, I had an opportunity of examining a portion of the spine, the intervertebral joints of which were preserved entire; upon sawing through two of the vertebræ, a fluid was met with, of the consistence of liquid jelly with clots of different sizes floating in it, so that in eight days a considerable tendency to coagulation had taken place, although the fluid was entirely excluded from the external air.

Coagulates after death.

The form of the cavity, thus exposed by a longitudinal section being made of it, is nearly spherical, capable of containing three pints of liquid; the lateral parts are ligamentous and elastic, uniting together the edges of the concave surfaces of the two contiguous vertebræ. When the liquid is evacuated, the elasticity of the lateral ligaments brings the ends of the vertebræ within an inch and half of each other; in this state the inner layers of the ligaments, which are less firm in texture than the outer, project into the cavity, and may be mistaken for a part of its natural contents; this portion when soaked in water swells out to a considerable size, the water readily insinuating itself between the membranous layers of which it is composed.

Cavity containing it.

The whole thickness of the lateral ligaments is about one inch, the external half of which is compact and elastic, the other appears to possess a very slight degree of elasticity. The appearance of the joint is shown in the annexed drawing, and an account of the analysis of the fluid by Mr. W. Brande forms a postscript to this paper, Every part of the mechanism is formed on so large a scale, that it is rendered conspicuous, and nothing is left to doubt or conjecture; the nature of the joint is different from every other that is met with in animal bodies, and there are many circumstances respecting it, which render it uncertain whether human ingenuity can ever make any resemblance to it, that can be applied to the purposes of mechanics.

Lateral ligaments.

The joint

These would have been sufficient grounds for bringing this subject before the Society; but there are others of still greater importance, which have induced me to make it a separate communication; it enables us to explain the general

enables us to explain the common principles of the structure of the spine.

ral principle upon which all intervertebral joints are formed, which has been hitherto but imperfectly understood. With this view, I will first describe the principle upon which this particular joint is formed, and then show the resemblance that it bears to those of other animals, in which the parts are not so readily distinguished from one another, and consequently their precise use has not been accurately ascertained.

Use of the fluid.

The fluid contained in the cavity, being incompressible, preserves a proper interval between the vertebræ, to allow of the play of the lateral elastic ligaments, and forms a ball round which the concave surfaces of the vertebræ are moved, and readily adapts itself to every change, which takes place in the form of the cavity.

Elasticity of the ligaments,

a substitute for muscular action.

The elasticity of the ligaments, by its constant action renders the joint always firm, independent of any other support, and keeps the ends of the vertebræ opposed to each other, so that the whole spine is preserved in a straight line, unless it is acted on by muscles or some other power. When a muscular force is applied to one side of the spine, it stretches the elastic ligament on the opposite side of the joint, and as soon as that force ceases to act, the joint returns to the former state. This is one of the most beautiful instances in nature of elasticity being employed as a substitute for muscular action.

The extent of the motion in each particular joint is undoubtedly small, but this is compensated by their number, and the elasticity of the vertebræ themselves.

Other fish have a similar fluid.

Fish in general have their vertebræ formed with similar concavities to those of the squalus maximus; these, when examined after death, contain a solid jelly, but in the living fish it is found in a fluid state. This fact was acertained in the skate, the smallness of the quantity of fluid in any one joint, and the readiness with which it coagulates after death, prevented it from being before observed: the fluid in the skate is found by Mr. Brande to have the same properties, as far as the small quantity that can be collected admits of examination, with that in the squalus maximus.

Form of the cavity differs

Although this structure of the intervertebral joint appears to be common to fish in general; the form of the cavity

vity is not in all exactly the same; in the skate it is very similar to that in the squali, but in the common eel it is more oblong, the longitudinal diameter being about one third longer than the transverse one.

It it evidently contrived for producing the quick vibratory lateral motion, which is peculiar to the back bones of fish while swimming, and enables them to continue that motion for a great length of time, with a small degree of muscular action.

Peculiarity in the spine of the sturgeon,

In the sturgeon, there are some curious peculiarities in the structure of the spine. Externally there is the common appearance of regular vertebræ, but these prove to be only cartilaginous rings, the edges of which are nearly in contact, and are united together by elastic ligaments, forming a tube the whole length of the spine; this is lined throughout its internal surface with a firm compact elastic substance, about the thickness of a cartilaginous tube; within this is a soft flexible substance in a small degree elastic; in the centre there is a chain of cavities in the form of lozenges, containing a fluid, and communicating with one another by very small apertures bearing a slight similarity to the intervertebral cavities of the spine in other fish.

and the lamprey.

As all the different parts of which this spine is composed are more or less elastic, except the central fluid, it must have great flexibility adapting it to the motions of this particular fish. The structure of the spine in the lamprey eel resembles that of the sturgeon.

Spine of the whale similar to that of quadrupeds.

The intervertebral joint, which is common to fish, is not met with in any of the whale tribe, whose motion through the water is principally effected by means of their horizontal tail; in them the substance employed to unite the vertebræ together is the same as in quadrupeds in general, and from the size of the vertebræ it is on a larger scale, and rendered more conspicuous.

A gelatinous substance instead of the fluid.

The external portion is very firm and compact, is ranged in concentric circles with transverse fibres uniting the layers together, it becomes softer towards the middle, and in the centre there is a pliant soft substance without elasticity, but admitting of extension more like a jelly than an organized

 body,

body, corresponding in its use to the incompressible fluid in the fish.

Other animals examined.

To ascertain whether this structure was generally met with in the spines of quadrupeds, Mr. Brodie, at my request, examined the intervertebral substance in a great many animals, and found, what, undonbtedly, was very little to be expected, that in the hog and rabbit, in the central part, there is a cavity with a smooth internal surface of the extent of half the diameter of the vertebra, in which is contained a thick gelatinous fluid, so that in some quadrupeds there is an approach towards the intervertebral joint in fish; but whether this is to answer any essential purpose to these animals, or is only to form an intermediate link in the chain of gradation of structures, which is so uniformly adhered to in the productions of nature, cannot at present be determined.

Hog & rabbit.

Bullock, sheep deer, monkey, and man.

In the bullock, sheep, deer, monkey, and man, the structure corresponds with that of the whale; in the three last, the central substance appears to be the most compact. Besides the structures already mentioned, there is in some animals one of a very different kind; in the alligator the vertebræ through the whole length of the spine have regular joints between them, the surfaces are covered with articulating cartilages; and there is synovia and a capsular ligament. In the snake, there is a regular ball and socket joint between every two vertebræ; so that the means employed for the motion of the back bone in different animals, comprehends almost every species of joint with which we are acquainted.

Allegator.

Snake.

Having mentioned a sufficient number of facts to point out the animals, in which the different structures of the intervertebral substance are to be found, I have abstained from being more particular in my account; as it would in no respect elucidate the principal object of the present communication.

Human spine.

From the facts and observations which have been stated, it appears that the intervertebral substance of the human spine does not consist entirely of elastic ligament, dense in its texture at the circumference, and becoming gradually softer towards the centre; but that the middle portion is

composed

composed of materials which render it very pliant, though not at all elastic, fitting it to keep the vertebræ at the proper distance from each other, so as to admit of the action of the lateral elastic ligaments.

Curvature of the spine.

When this knowledge is applied to the treatment of curvatures of the spine, a complaint so commonly met with in young women, whose strength does not bear the necessary proportion to the growth of the body, it will show the great impropriety of overstretching the intervertebral ligaments, since in that state the central substance no longer supports the vertebræ, and the joints must lose their proper firmness, which will be attended with many disadvantages.

Advantages of comparative anatomy.

As the principal motive, which induces me to prosecute the laborious researches of comparative anatomy, is to attain a more complete knowledge of the structure and functions of the human body, than can be acquired in any other way; and to apply this knowledge to the most useful of all purposes, the cure of dieases; the success which has attended my labours, in the present instance, affords me particular satisfaction; it encourages me in the pursuit of those inquiries, and holds out an invitation to others, by showing them that the paths of nature, however frequently they have been traced, are not yet sufficiently explored.

Explanation of the Plate.

Explanation of the plate.

A longitudinal section of one of the intervertebral joints of the squalus maximus, after the fluid had been evacuated, and the parts had been steeped in water.

Pl. V, *a a a a*. The section of the vertebra, to show its shape and the two concave surfaces which form the intervertebral cavities. The vertebra itself is partly bone, and partly transparent cartilage; the bony portion forms the two cup-like cavities, and the intermediate substance consists of bony cells in form of lozenges filled with cartilage.

The cavity of the joint is in its contracted state, and the inner portion of the lateral ligaments, which is made up of thin layers of a loose texture, has its interstices loaded with water, which makes it project into the cavity of the joint more than it could do in a natural state.

The

The external portion of the ligament, to the thickness of half an inch, is the only truly elastic part on which its strength depends.

A chemical Analysis of the Fluid contained in the intervertebral Cavity of the Squalus Maximus. By Mr. WILLIAM BRANDE.

Analysis of the intervertebral fluid of the spine.

The fluid found in the intervertebral cavities is of an opal colour; it is semitransparent, and has a strong fishy smell and taste.

Its specific gravity is 1·027.

In the first instance it does not readily mix with water; but is easily diffused through that fluid by ignition.

When heated in a water bath to a temperature of 212°, it becomes more transparent, but undergoes no farther apparent change.

Infusions of galls and of catechu produce no alteration in it.

Solution of oximuriate of mercury occasioned a very copious white precipitate, and a similar effect was produced by a solution of nitro-muriate of tin.

Nitrate of silver and acetate of lead threw down precipitates of muriate of silver and of lead.

Muriatic acid occasioned a slight cloud after two hours had elapsed, and after twenty-four hours, a small quantity of white flaky matter separated.

Alcohol produced no change.

The fluid readily mixed with a solution of pure potash, a small quantity of ammonia being at the same time evolved. Muriatic acid did not produce any immediate precipitation in this alkaline solution.

The effect of these reagents evidently proves the non-existence of gelatine in this fluid; it would also appear, that it contains no albumen, unless the effects produced by muriatic acid, and by the oximuriate of mercury and of tin, be regarded as indications of that substance.

It seems to approach nearer to *mucus* or *mucilage*, than to any other animal fluid*. When

* By *mucus of animals,* I mean a glary fluid, which does not mix readily

Analysis of the intervertebral fluid of the shark.

When the fluid is evaporated in a temperature not exceeding 220° to half its bulk, an opaque substance, in the form of blueish white filaments, gradually separates. A thin semitransparent pellicle forms at the same time upon the surface, which, when removed, is soon succeeded by another. These pellicles were dried on bibulous paper.

The fluid part, remaining after the separation of the filamentous substance and pellicles, afforded a very distinct yellowish cloud with solutions containing tannin. It was somewhat turbid, but did not form any deposit. In other respects, it nearly resembled the original fluid before evaporation.

The filaments, which appeared during evaporation, were separated by passing the fluid through a piece of fine muslin. They resembled albumen imperfectly coagulated, not only in appearance, but in most of their chemical properties.

When the fluid began to putrify, a considerable quantity of the same substance separated spontaneously.

This substance was insoluble in water, and when boiled for a few minutes in that fluid, it became whiter, harder, and more opaque.

It underwent the same change in alcohol, and when boiled in alcohol, or in dilute muriatic acid, it became still more firm, and appeared like perfectly coagulated albumen.

In this state it was soluble in a solution of pure potash, forming a saponaceous compound, which was decomposed by dilute muriatic acid, a white flaky precipitate being formed. It possessed the other properties, which Mr. Hatchett has enumerated as belonging to coagulated albumen †.

When the pellicle, which had formed on the surface of the fluid during evaporation, was nearly dry, it became somewhat tough and elastic; it was semitransparent, and of a dirty white colour.

dily with water, which is coagulated neither by heat nor acids, and which does not form a precipitate with solutions containing tannin.

† Vide Phil. Trans. 1800.

When

Analysis of the intervertebral fluid of the shark.

When boiled for some time in water, about three fourths of it were found to be soluble in that fluid, the remainder, when separated by filtration, possessed the properties of the albuminous substance already mentioned.

The solution afforded a copious precipitate with solutions containing tannin. It was not at first affected, either by oximuriate of mercury, or of tin; but after twenty-four hours, a slight deposit took place.

Although these reagents indicated the presence of a substance having the properties of pure gelatine in solution, yet it could not be brought to gelatinize by the usual method of evaporation.

From these experiments it would appear, that the intervertebral fluid is of a peculiar nature; that in its original properties it resembles mucus, but that under certain circumstances it is capable of being converted into modifications of gelatine and albumen.

The intervertebral fluid of the skate was found to resemble mucus; it did not exhibit any traces of albumen, but the quantity which I procured for examination being very small, I was unable to ascertain its further analogies to the fluid found in the intervertebral cavities of the squalus maximus.

XI.

On expectorated Matter. By GEORGE PEARSON, *M. D. F. R. S.**

States of the fluids too much neglected from the humoral pathology having fallen into disrepute.

THE attention of physiologists has been very much withdrawn, for the last half century, from the consideration of the different states of the circulating and secreted fluids, in consequence of the opinion, that the nervous and fibrous or muscular systems can afford satisfactory interpretations of the phenomena of living beings; and on account of the disgust produced by the visionary properties and groundless hypotheses, originating in the humoral doctrines of Galen.

* Phil. Trans. for 1809, p. 313.

But

But late experiments have manifested, that various things taken into the stomach can be made at pleasure to produce considerable effects, by impregnating sensibly the blood and urine, as well as the milk, sweat, and perhaps saliva. Further; the fine experiments of professor Colman have shown, that the contagious glanders may be excited in the ass by the transfusion of the blood of a glandered horse, and the matter from the nose of the glandered ass can produce this disease in the horse or the ass*. Hence I apprehend it is reasonable to expect, that the farther investigation of the properties of the animal fluids will afford gratifying instruction to the researcher in natural science, and important practical information to the physician.

Instance of the importance of their effects.

On the present occasion, I desire the honour of communicating the knowledge I may have acquired by investigating the properties of expectorated matter secreted by the bronchial membrane. The appearances of this substance serve to regulate the judgment of the physician concerning several diseases of the lungs; but especially of that of pulmonary tubercles, which yearly destroys from 120000 to 140000 subjects of the United Kingdom. It is fit that I remark, that I do not notice in this paper the ingenious experiments of several learned chemists, because by so doing I should be led into a detail of too great extent for my design.

Importance of a knowledge of expectorated matter.

The numerous varieties of expectorated matter, according to my observation, may be arranged and characterized under the following seven heads:

I. The jellylike semitransparent kind of a blueish hue, excreted in the healthy state.

Its varieties.

II. The thin mucilagelike transparent matter, so copiously expectorated in bronchial catarrhs.

III. The thick opaque straw coloured, or white and very tenacious matter, coughed up in a great variety of bronchial and pulmonary affections; especially in that of tubercles.

IV. Puriform matter secreted without any division of continuity,

* Mr. Colman alleges, that there is not a sufficient quantity of blood, in a single glandered ass, to excite the glanders by the transfusion of blood into the horse.

tinuity, or breach of surface of the bronchial membrane, very commonly occurring in pulmonary consumptions.

V. The matter which consists of opaque viscid masses, together with transparent fluid; or the second sort above stated, with nodules of the third or fourth kind.

VI. Pus from the vomicæ of tubercles.

VII. Pus from vomicæ by simple inflammation of the lungs, and without tubercles.

Other kinds.

Other kinds of matter are occasionally coughed up, such as calculi—masses of self-coagulated lymph—serous fluid —blood itself—and perhaps the vascular substance of the lungs; but I do not write on these matters, because they either do not belong to any particular recognized disease, or they are rare occurrences in some well known disease, and are too obvious to require description.

SECT. I. *Sensible, or obvious Properties.*

Jellylike blueish matter.

I. *The jellylike matter*, as already said, is excreted in the best health, as well as sometimes in disease. It is mostly coughed, or hawked up, in a morning, soon after a night's repose, during which it seems to accumulate. A few masses, or nodules, then appear of the consistence of jelly, and from the size of a pea to a hazle nut. It is also at any time liable to be excreted, in consequence of various extraneous matters irritating the fauces, to the amount of a few nodules. It is of a grayish colour, or inclining to blue, with black specks; and it is rarely whitish in nodules. The consistence is that of jelly, but of much greater tenacity. It has a barely perceivable taste of common salt, or muriate of soda. It commonly floats on water, but by agitation to disengage air bubbles, it sinks. It has no smell. To the naked eye, or assisted by a single magnifier, this matter seldom appears uniform, but consists of a mixture of opaque and transparent masses of irregular figures. With the compound microscope, spherical particles were perceived, though few in number, when duly diluted. The presence of an alkali I could in no instance perceive, by means of the usual tests, *namely*, turmeric paper, litmus paper slightly reddened by vinegar, and cloth stained with violet juice; nor was an acid denoted by means of litmus paper, except when

I had

I had reason to believe it was derived from various acid substances taken with the food, or drink, adhering to the inside of the mouth and fauces.

Mucilagelike matter.

2. *The mucilagelike expectorated matter*, according to my observation, occurs much less frequently than the other sorts. It appears suddenly in great abundance in certain bronchial catarrhs. I have seen it to the amount of two or three pints in twenty-four hours. It is also secreted, but less copiously in paroxysms of spasmodic asthma, and of the hooping cough; and but rarely in pneumonic, or pleuritic inflammations, and in some chronical organic diseases of the heart and lungs.

Described.

This matter is a transparent uniform fluid of the consistence of white of egg; or of a mucilage compounded of about one part of Arabic gum, and four or five parts of water. It is colourless—has a fleshy smell—has a brackish taste. After standing eight or ten hours, a deposit takes place of fibrous, leaflike, or curdy masses, some of which are seen suspended in the clear fluid. In some cases nodules of opaque thick ropy matter, at certain times, accompany this mucilagelike matter. Under the simple magnifier I perceived irregular figured masses partly in motion and partly suspended, With the microscope, globules were seen; but larger considerably than those of the blood, and much less numerous. With the usual tests there were no indications of alkali, or of acid, provided the matter was unmixed with other things. It usually floated, or was suspended in water, when first expectorated; but on standing in the water it fell to the bottom, evidently owing to the disengagement of air bubbles.

By standing exposed to the air in warm weather, it sooner grew fœtid than pus of abscesses; without becoming opaque. Neither could I render it opaque or thicker, by exposure to a stream of oxigen gas for an hour; or by exposure of it in a jar of this gas for a month.

3. *The opaque ropy matter* above-mentioned,

Opaque ropy matter.

1st. It is secreted most copiously in that very common, and extensively epidemial disease of our climate, the *winter-cough*, occasioned by tubercles, to the amount of half a pint or a pint in twenty-four hours; especially during the winter

season

season for several successive years, and sometimes during the whole of a long life, after the age of forty or fifty years. 2dly. It is often the expectorated matter of the pulmonary consumption of young persons, also occasioned by tubercles, but frequently mistaken for the pus of abscesses or vomicæ. 3dly. It appears, oftentimes, in pneumonic or bronchial inflammation with fever, seemingly being a beneficial discharge; as well as in some instances at the close of a fever without concomitant inflammation of the lungs. 4thly. A severe paroxysm of spasmodic asthma is often terminated in the excretion of this kind of matter. 5thly. A secreted substance of this sort is sometimes expectorated in various chronical organic diseases of the lungs, the heart, aorta, and parts contiguous to the lungs, which occasion difficult transmission of blood through them.

Described

In all these instances the matter by expectoration is of the consistence of thick cream, or of thin toasted cheese; so tough as to hang in the form of a rope, four or five inches in length, on pouring it from one vessel into another. Its aggregation is such, that it is readily detached in large masses from the vitreous surface of vessels. It is not unusual for small black, or reddish spots, and streaks, to appear on the surface of this sort of expectorated substance. A pretty large bulk of it is seldom throughout uniform; but it is frothy, and exhibits opaque masses of various hues with transparent matter interposed. The *colour* is yellowish, straw-coloured, and white, or gray: it also, though seldom, is greenish and bluish. The *taste*, asserted by patients, is, in their own terms, various, *namely*, saltish, nasty, faintish, sweetish, luscious, or like that of a sweet oyster,—a sharp or sour taste is the most rare. The only *smell* which I have perceived is that of flesh, but very frequently there is none. When any offensive or pungent smell was perceived, immediately after expectoration, I have always found that it was owing either to the foulness of the vessel in which it was received; or it was from extraneous matters in the mouth, and from decayed teeth.

Circulating spherical globules.

This opaque viscid substance, being duly diluted with distilled water, was examined with microscopes of common, as well as of very great powers: by means of any of them crowds

crowds of spherical particles were seen passing to and fro, in currents, not unlike those of the blood; except that they were larger. These globules I could not destroy, or alter in form, by trituration; or by long boiling in water; or by exsiccation, and again dissolving in water; or even by coagulation with mineral and vegetable acids, with alcohol, with sulphuric ether, or with tannin, and alum; or by mixture with caustic alkalis in a proportion which leaves the liquor turbid; or for some time after the putrefactive process had appeared. But these globules disappear with such a proportion of sulphuric acid as detaches charcoal; or of nitric acid, and of liquid potash, as produces a clear solution: also by charring by fire. It is perhaps superfluous to remark, that these atomic globules are quite different from the air bubbles usually entangled in this kind of matter, as perceived by the microscope; the latter differ much from the former; in being of far greater magnitude—in being less numerous—in being transparent, and disappearing on agitation, or heating the matter, or even by mere standing.

For the most part this expectorated substance swims on water; but by agitation or stirring to disengage air bubbles, or by merely standing, it sinks. Some of the lumps suddenly hawked up immediately fall to the bottom of a vessel of water. No signs of either acid, or alkali, appeared on the trials of this matter with well known reagents, provided it was free from extraneous matter; but it was apt to betray acidity from things taken with the food or drink.

Puriform matter.

4. *Puriform matter.* I have seen this matter expectorated in several diseases in the quantity of two or three ounces to half a pint in twenty-four hours, on some rare occasions, without any breach of surface. I believe it would be considered by every one to be *pus*, having the properties commonly admitted to be those of this substance. It will however, perhaps, only be just to call it *puriform*, for the present, as it appears to me probable, that I shall hereafter be able to show, that it possesses properties not belonging to pus of abscesses, although in the obvious, or sensible properties, it is similar to such pus. Accordingly this expectorated matter is not only opaque, white, or yellowish, and as thick as the richest cream, but it also has not more tenacity

city than cream. It is not apt to entangle air, and therefore it immediately mingles with water, rendering it milky; and presently subsides to the bottom, leaving the water clear, or at least whey-coloured. It appears to the naked eye uniform in its texture; and nearly so under the simple lens: but under the microscope thousands of globules similar to those of the blood are seen, which are indestructible as those above related belonging to another kind of expectorated matter.

Not from ulceration.

The substance, of which I am now speaking, is most frequently excreted in the latter stages of pulmonary phthisis, for many weeks successively. It is taken for granted, that this matter is from a breach of surface or ulceration; but on examination after death, such a state was not found, in many instances, under my observation, although the lungs were as usual full of tubercles and vomicæ. This puriform matter is occasionally expectorated in certain other diseases. The last summer my colleague, Dr. Nevinson, furnished me with several ounces of this sort of substance, but of a greenish hue, and of the consistence of thin cream; which was expectorated by a woman in the third week from the attack of the measles. In a few days she died. On the examination of the lungs very carefully, by the excellent house surgeon of St. George's hospital, Mr. Dawes, no ulceration could be discovered in the trachea or in the bronchial tubes; nor were any tubercles, or abscesses found in the lungs. The patient, according to my information, had expectorated more than a pint of this fluid every twenty-four hours for a week before death. In another hospital case, a man laboured under a cough with spitting of matter, which all who saw it called pus, and as usual it was considered to arise from an ulceration, or suppurated tubercles; but, on examination after death, the disease was ascertained to be condensation of the lungs to the consistence of liver, with water in the cavities of the chest, and nothing more.

Opaque viscid matter.

5. *Opaque viscid matter of the third, and perhaps fourth sort,* above distinguished, appearing in nodules, and irregular figured masses, *mixed with transparent slimy matter of the second sort.*

It is not unusual to see the mixture of these two different

ferent kinds, from severe fits of coughing in that constant epidemy of the British islands, the winter chronical pneumonia.

Different parts of the bronchial membrane being in different states may account for the secretion of the two different matters. This seems more probable, than that these different matters should be secreted from the same part; although it is true, that the same part does secrete at one period transparent thin slime, and at another an opaque thick matter. The former is occasioned by great irritation of the membrane, and the latter is the effect of a more gradual secretion with much less irritation.

For the sake of brevity I avoid a further description. The practical application of these observations, however important, would or be suitable in this place.

The sixth and seventh kinds of expectorated substances being secreted after a quite different manner, and being very different in their nature from the preceding five kinds, I shall not give an account of them in this paper.

SECT. II. *Agency chiefly of Caloric.*

Chemical analysis. Action of heat.

1. No effect of importance is produced by this agent, until the temperature of the expectorated matters is raised to about 150° of Fahrenheit's thermometer: then the state of aggregation is evidently altered, the viscidity of each of them being diminished. At about 155°, coagulation begins to be quite evident in the first, third, fourth, and fifth kinds of matter—that is, curdy masses of various magnitudes appear in a milklike or whitish liquid. On elevating the temperature to 160° or 170°, a large proportion of curd is formed; but the proportion of the curdy matter to the liquid is very different in different specimens. The viscid texture, or tenacity of the expectorated matters, is by this treatment destroyed. The milky liquid decanted, after standing ten or twelve hours, affords, on evaporation to dryness, about three or four grains of residue from each 100 grains.

This liquid passes very slowly through the paper filter. The filtrated liquor affords scarcely more than one per cent on evaporation to dryness. By repeatedly boiling in successive

cessive portions of water, the whole, as far as I could judge, of a given quantity of the curd might be diffused to form a whitish liquid; which on evaporation to dryness appeared to afford a residue of the same kind (except in containing a smaller proportion of saline substances), as the milky liquid which was separated from the curd on the coagulation of the expectorated matter.

The second kind, called *mucilagelike transparent matter*, does not afford curdy masses at the temperatures abovementioned; but its viscid texture is destroyed, and it becomes a wheylike, or somewhat milky liquid; and, on examination with a magnifying glass, it appears full of curdy particles. After this agency of caloric, the expectorated matter is much less prone to putrefaction.

Distilled fluid. "2. Distillation of the expectorated matters to dryness afforded a particularly limpid water, which had a peculiar smell, but no impregnation with ammonia; or with any other substance which could be detected, except a little carbonic acid.

Residuum. The residuary matter, in a brittle state of dryness, afforded by evaporation, varied between two and a half and ten per cent of the expectorated substances. The second kind yielded from one thirty-fifth to one forty-fifth of its weight of brittle residue. The first kind afforded from one twentieth to one twenty-fifth of residue. The third kind afforded very different proportions of solid residue, according to its consistence, viz. from one tenth to one eighteenth of its weight. The fourth kind gave from one twelfth to one fourteenth of brittle matter. The fifth kind yielded very different proportions of residue, according to the very different proportions of transparent and opaque matter, of which it consisted—it varied between one eighteenth and one thirtieth.

Attracted moisture from the atmosphere. 3. All these exsiccated substances on exposure to air grew more or less moist, or at least were no longer brittle; but became somewhat soft, and proportionately to the state of moisture, were augmented in weight. The thinner the expectorated matters, the moister, and the greater increase of weight they generally experienced. But parcels of the same consistence from different patients sometimes differed much

much in degree of moisture on exposure to the air. I have found some parcels of the second and fifth sorts of expectorated substances grow quite moist, and receive an increase in weight of three per cent. If the residues were kept in close vessels, they remained in a brittle state. Larger parcels of exsiccated matter become more moist than smaller ones of the same kind in the same circumstances.

Milky and curdy liquids, and curdy matter exsiccated.

4. The milky and curdy liquids, which separated from the curdy masses (1) being poured off; and also the curdy masses being by pressure rendered dry; the liquids were evaporated to dryness, but became moist on exposure to the air. The curdy masses were by evaporation rendered brittle, and remained so in the air. The residues of the evaporated liquids were said to taste extremely salt, and the exsiccated curdy matter was tasteless.

Milky liquids.

5. The milky liquids (4), concentrated by evaporation, did not indicate any disengaged acid, or alkali, to the usual reagents.—By triturating these liquids with lime, a little ammonia was discharged—by trituration with concentrated sulphuric acid, the muriatic acid was disengaged—with phosphoric acid, and also with tartaric acid on trituration and heating, a pungent smell was perceived, somewhat like that of the acetous acid. On burning to a brown ash the saline residue afforded by evaporation of these liquids, the predominating taste of it was that of muriate of soda. This ash readily melted,—being moistened, it turned turmeric paper to a reddish brown colour, and changed turnsole paper, reddened by acetous acid, to a deep blue—on exposure to the air, it partially deliquesced—the dissolution, by boiling in distilled water, afforded supertartrate of potash on the addition of the tartaric acid; and a red precipitate was occasioned by nitro-muriate of platina*. This incinerated and fused saline residue by other trials was proved to contain phosphoric acid and lime; with traces of sulphuric acid, magnesia, iron, and perhaps silica; but the chief ingredients were muriate of soda and potash.

Curdy matter.

6. The curdy matter after expression (4) afforded a much smaller proportion of brown ash than the fusible sa-

* The knowledge of this reagent, I believe, the chemical world owes to Dr. Wollaston.

Opaque ropy matter.

line residue (5). It required an intense fire for fusion in a platina crucible. The fused mass did not deliquesce, but it grew somewhat moist on exposure to the air. It contained a much smaller proportion of potash than the former fused matter (5); also much less of muriate of soda, but a far larger proportion of lime and phosphoric acid, with traces of sulphuric acid, magnesia, oxide of iron, and perhaps silica.

7. (*a*) 15400 grains of the third sort of expectorated matter on exsiccation afforded 960 grains, that is, one sixteenth of brittle substance, or about six per cent, and of course this kind of matter contained about ninety-four per cent of water (sect. II. 2). This dried matter was reduced to a charred state by exposing it to fire in a Wedgwood white crucible. In this process it inflamed, emitted the usual smell of burning animal matter, especially of bone, and swelled prodigiously; at the same time a black oil was compounded rendering the mass soft during the inflammation. I could not distinguish the smell of sulphur, but there was in one part of the burning, a smell, to my sense, of phosphorus.

(*b*). This charred matter was kept in a state of ignition in a platina crucible, till it no longer remained in a powdery form, but was reduced to a comparatively small bulk of a substance of the consistence of paste in an intense fire. By continuing the fire, the charge at length was fused; and after being kept in a state of fusion to be quite fluid for ten minutes, the fire being withdrawn, a white, brittle, apparently saline matter, like melted common salt, was easily detached from the platina crucible, which in some places had received a red tinge.

(*c*). The melted matter (*b*) weighed fifty-nine grains: of course, this saline residue amounted to $\frac{1}{261}$ of the expectorated matter, and to one sixteenth of this expectorated matter exsiccated. It tasted only of muriate of soda—it had no smell—it effervesced with acids—it betrayed the presence of alkali to the tests above-mentioned—after a few days exposure to the air, it partially deliquesced—it precipitated supertartrate of potash with tartaric acid, and emitted no ammonia with lime, or sulphur with muriatic acid, discoverable by the most delicate tests.

(*d*). The

Opaque ropy matter.

(*d*). The fused matter (*c*) was boiled with three times its weight of distilled water, in which about one half appeared to dissolve. The clear liquid, decanted from the sediment and evaporated, yielded crystals of muriate of soda with a much smaller quantity of spicula, or needle-shaped crystals; and saline matter which appeared under a lens not definitely crystallized. A second boiling of the sediment, with twice its quantity of water, afforded almost entirely muriate of soda. A third boiling gave a few crystals of this salt only, as appeared under the magnifier. A fourth boiling, in an equal weight of water, afforded no saline matter.

(*e*). The saline matters (*d*) amounted to forty-five grains when evaporated to dryness. I collected by means of a toothpick, from amongst the cubical crystals, as much as I could of the spicula and uncrystallized saline matter. These parts effervesced and precipitated supertartrate of potash with tartaric acid, and certainly afforded no soda-tartrate of potash—they also afforded a precipitate with nitro-muriate of platina—being saturated with acetous acid there was still a slight precipitation with muriate of baryt; for without acetous acid, there was a most copious precipitation with this reagent, but the greater part of the precipitate was dissolved by acetous acid, added so as not to supersaturate it.—Oxalate of ammonia did not occasion a precipitation,—with nitrate of silver an abundant one took place—lime water produced only slight turbidity. The muriate of soda amounted, in this saline mass of forty-five grains, to thirty-five grains, or nearly to one grain in 460 of expectorated matter; the rest was subcarbonate of potash amounting to one grain, in about 1540 grains of expectorated matter, with which was mixed a minute proportion, probably, of sulphate and of phosphate of potash.

(*f*). The undissolved matter (*d*) boiled with muriatic acid gave a turbid liquid, but on standing, nearly the whole appeared to have been dissolved: a small proportion of sediment only took place in a transparent liquid, which was boiled till it no longer parted with muriatic acid.—This dissolution being exsiccated grew liquid on exposure to air; and oxalate of ammonia, gradually added, produced, as I decidedly ascertained, the precipitate of oxalate of lime.

 (*g*). The

Opaque ropy matter.

(*g*). The filtered residuary liquid (*f*) with muriate of Baryt gave immediately a copious precipitation—with lime water there was milkiness produced, and subsequently a white precipitation which did not disappear on adding a small proportion of acetous acid—prussiate of potash occasioned a greenish blue colour without precipitation—succinate of ammonia produced a milky liquid—no effect was observed from tartaric acid—There being a precipitation with caustic or pure ammonia, as well as with potash, and with the carbonates of the alkalis, it was supposed magnesia was present: and the dissolution of this precipitate in muriatic acid, and in acetous acid, gave no precipitate with oxalic acid. Some of the muriatic dissolution, previously to precipitation with oxalate of ammonia (*f*), being evaporated to dryness, the residue was ignited; but if magnesia was present, as well as lime, it was in too small quantity to be distinguishable from the lime, by composing sulphate. The precipitate now under examination was certainly not mere magnesia, for it melted into an opaque globule under the blowpipe; it was not phosphate of lime, for with sulphuric acid a somewhat bitter and sour substance was compounded, which afforded a precipitate with ammonia, but none with oxalate of ammonia. It was a phosphate not only on account of its fusibility, but because a curdy appearance was occasioned by the mixture just mentioned with sulphuric acid, on adding it to lime water. Neither was it soluble, like phosphate of lime, in phosphoric acid. The quantity of this precipitate was too minute for decisive experiments, but from these related, it seems probable that it was phosphate of magnesia, which was dissolved, as will appear presently, in phosphoric acid, and precipitated by ammonia.

(*h*). The residuary liquid (*g*), after the precipitation by oxalate of ammonia, being evaporated to dryness, was easily ascertained to be phosphate of ammonia, with indications of a minute proportion of sulphate.

(*i*). It remains only to notice the indissoluble matter in muriatic acid (*f*). I found it to grow soft, and the parts to cohere under the blowpipe, and with a little potash it readily melted into an opaque globule.

8. To

8. To obtain a more satisfactory proof of the presence of sulphur, forty grains of charred expectorated matter were kept in a state of ignition in a platina crucible, with another inverted over it to exclude completely the escape of gas, for two hours. After cooling, the smell of sulphuretted hidrogen gas was very evident, on the addition of diluted muriatic acid, and even of water only. Silver was tarnished, and paper wetted with liquid acetite of lead was blackened by this gas. In some of the experiments, while the charcoal was burning off from the charred expectorated matter, I perceived the smell of sulphur, and perhaps of phosphorus.

(To be concluded in our next.)

XII.

On the Nonabsorption of Oxigen in Respiration, in Reply to Mr. Acton, *by* J. F.

To Mr. NICHOLSON.

SIR,

Defence of Mr. Ellis against the charge of self contradiction.

MY former remarks on Mr. Acton's communication were designed to rescue Mr. Ellis from the imputation of "perverting the experiments" of Mr. Bichat.—My present aim is to defend the same writer from the charge of contradicting himself. "If," says Mr. Acton, "two experiments of an opposite nature, mentioned doubtless to prove the truth of the general proposition, can be considered a contradiction, then has Mr. Ellis contradicted himself." To this I reply, that a general proposition may be supported by dissimilar proofs, which require experiments of an opposite nature, without involving any contradiction: and such I take to be the case in the experiments referred to by Mr. Acton. To prove, that air does not naturally enter into the blood, Mr. Ellis quotes an experiment of Hales, who injected air into the lungs of an animal, but it did not enter the vessels. As a farther argument against this opinion, Mr.

Mr. Ellis also states, that, if air be made to enter the blood, it quickly destroys life; and he refers for proof of this to an experiment of Mr. Bichat, who not only, like Dr. Hales, injected air into the lungs, but *confined* it there, by which means it did enter into the blood vessels, and speedily proved fatal. Where now is the contradiction in all this? Two different positions, tending doubtless to the support of one general proposition, were, by these experiments to be established; and, if the experiments be deemed correct, they go directly to fulfil their destined purpose. In his zeal against the general proposition, Mr. Acton seems to have overlooked the varying nature of the proofs adduced in its support, and he thus sees contradiction in opposite experiments directed to the establishment of dissimilar truths.

Messrs. Allen and Pepys found the oxigen lost in respiration converted into carbonic acid.

In my postscript I observed, that the late experiments of Messrs. Allen and Pepys seemed to support Mr. Ellis's opinion, that "all the oxigen gas lost in respiration was to be found in the carbonic acid produced." As Mr. Acton pronounces this an "inaccurate and unfair statement," I must beg leave, in justice to myself and to those gentlemen, to deliver a short abstract of their experiments nearly in their own words. In the analysis of the respired air of thirteen experiments, in each of which nearly from 3 to 4000 cubic inches of air were *once* passed through the lungs, and in one instance little less than 10000, where "the breathing was nearly natural, the operator scarcely fatigued, and his pulse at the end of the experiment (which lasted from 5 to 24 minutes) not raised more than about one beat in a minute," these gentlemen found the quantity of oxigen gas and carbonic acid, taken together, to amount always to 21 parts in the hundred, which exactly corresponded to the proportion of oxigen gas, previously ascertained to exist in the respired air: wherefore they concluded, "that the quantity of carbonic acid gas emitted is exactly equal, bulk for bulk, to the oxigen consumed." This conclusion is just what it should be, a simple expression of the fact, and nothing more; and it goes entirely to support the opinion of Mr. Ellis, to which I before alluded.

In some cases however oxi-

But in two other experiments, which lasted only two or three minutes, where not more than 300 cubic inches of air were

were employed, which were passed *eight* or *ten times* through the lungs, "until respiration became extremely laborious, with a great sense of oppression and suffocation in the chest, indistinct vision, buzz in the ears, and at last perfect insensibility," the oxigen and carbonic acid expelled amounted together only to 14 or 15 parts in the hundred, instead of 21, as in all the former experiments; and therefore when, as in these experiments, "respiration is attended with distressing circumstances, there is reason," say these gentlemen, "to conclude, that a portion of oxigen is *absorbed*." This conclusion, however, unlike the former, evidently involves matter of *opinion*, as well as matter of *fact*. To the fact, that a portion of oxigen was lost, I readily assent; but to the opinion, that this oxigen was absorbed, I must beg leave to demur. The two series of experiments differ not less in their chemical results, than in their effects on the animal system; and although these gentlemen, at the commencement of their excellent memoir, seem to have been fully aware, "that the deficiency in the respired air principally arises from the difficulty in bringing the lungs precisely to the same state after as before the experiment," yet they appear to have passed by this consideration in forming the above conclusion; and have thus been led to consider the mere disappearance of a portion of the inspired air as a proof of its *absorption by the blood*, when, in reality, it proves nothing more than its *retention in the lungs*.

gen disappeared.

This supposed to have been retained in the lungs.

I have thus endeavoured to separate fact from opinion in these experiments; and, if I mistake not, the fact is with me and Mr. Ellis—the opinion with Mr. Acton.

J. F.

REMARKS.

As it is but equitable, that every writer should be allowed to defend the opinions of himself or his friend, as far as may be done without indulging endless or unnecessary controversy, the preceding paper has been admitted: though I apprehend nothing remains now to be said on either side, unless

Controversy requires limitation.

unless fresh facts can be adduced, that may tend to decide the point at issue.

The oxigen disappearing in some cases of respiration not simply retained in the lungs.

I would just observe however, that J. F.'s mode of accounting for the deficiency of the proportion of oxigen in the last two experiments of Messrs. Allen and Pepys quoted by him appears to be founded on an erroneous idea. Those gentlemen did not ascertain the absolute quantity of oxigen in the air after respiration, they merely determined the proportions of oxigen and carbonic acid to nitrogen in a given quantity; consequently, whether the air left in the lungs at the end of the experiment were more or less, this air would have nearly the same proportions of oxigen, carbonic acid, and nitrogen, as that in the gasometer; so that no surplus in the lungs would account for the deficient proportion of oxigen.

The mode of conducting the experiment shows this.

There is another remark may be made on this subject. The operator, in the experiments of Messrs. Allen and Pepys, draws the air into his lungs from one vessel through one tube, and then breathes it out through another tube into another vessel; and this he continues to repeat a considerable number of times, all communication between his lungs and the external air being completely cut off both in expiration and inspiration. Consequently all the loss of quantity in the result, that can be ascribed to the lungs being less completely emptied at one time than another, must be the surplus, that the lungs retain after the last expiration above what they retained after the expiration previous to commencing the experiment. In the third experiment of Messrs. Allen and Pepys [see Journal, vol. XXII, p. 183] this loss is only four cubic inches, which might be ascribed to such a cause: but in several others it is upwards of thirty, and in the eighth the loss is sixty-two cubic inches, which is surely much too great to be accounted for in this way. I am inclined to suspect, that Messrs. Allen and Pepys themselves, when they ascribe the deficiency "principally to the impossibility of bringing the lungs to the same state after forcible expiration" [ib. p. 187], divided the quantity thus disappearing among the total number of expirations made; for this remark is introduced by the following words: "The smallness of the deficiency

Apparent miscalculation of Messrs. A.&P.

[18 cub.

[18 cub. inches], notwithstanding the experiment occupied 24½ minutes, &c." But the duration of the experiment could have no effect on the result as far as it was owing to the cause in question, since this is simply the difference between the air retained in the lungs previous to the experiment and after its conclusion, whether the experiment consisted of a single inspiration and expiration, or of a thousand. C.

XIII.

Observations on the Pickle of Violets, considered as a Reagent; and on the Advantage of Salting Vegetables, from which Distilled Waters are intended to be made: by Mr. DESCROIZILLES, *Sen.**

Sirup of violets apt to spoil.

IN chemical analyses, when we wish to detect the presence of uncombined acids or alkalis, or of alkaline subcarbonates, the most usual test is sirup of violets. But this has some inconveniencies, as it is very apt to spoil. Accordingly it occurred to me to try what I call pickle of violets, and I found it answer extremely well. The following is my mode of making it.

A saline infusion preferable

Mode of preparing it.

On the petals of the violet slightly pressed into a small pewter measure, pour double their weight of boiling water, and stir them together. Cover over the measure, and expose it for a few hours to a heat somewhat greater than that of a waterbath: after which let the liquor be strongly pressed out through a very clean linen cloth. Weigh the infusion accurately, and add to it one third of its weight of common salt, stirring it till this is dissolved. Very fine white salt should be chosen, as this contains little or no earthy muriate, which would be detrimental to the colour. In a small phial corked this will keep without alteration, though exposed to various degrees of temperature, and even to the

* Annales de Chimie, vol. LXVII, p. 80.

rays

rays of the sun. As a reagent it is far preferable to the best sirup of violets *.

Other blue flowers would answer.

It may be presumed, that several other blue flowers, as those of the iris, larkspur, &c., would afford a pickle of sufficient sensibility. The latter I have tried with success.

Method of using it.

To use this blue pickle, dip into the phial the end of a little stick, or of a match with the brimstoned part broken off; and with this end touch a clean earthen plate in various places. On one plate you may make thirty such spots for trial, each of which will not require above a quarter of a drop: so that a few drachms of this pickle will last a twelvemonth, though you have frequent occasion for its use.

Vegetables might be preserved for distillation by salting.

It appears that the utility of common salt in preserving vegetables required to be brought from a distance, for the use either of the apothecary or perfumer, has not been in general sufficiently understood. Hilary Marin Rouelle, whose pupil I was, perfumed his laboratory during a whole course of chemical lectures in the winter of 1775 by distilling roses he had salted in the month of june. The rosewater he obtained, being mixed with sugar and alcohol, formed a delicious cordial. I have kept a jar full of salted roseleaves in my laboratory these three years, and their perfume has lost nothing of its strength or sweetness. They may be salted in the following manner.

Process of salting.

Take a kilogramme and half [4lbs. troy] of roseleaves, and bray them for two or three minutes with one third their weight of common salt. The flowers, bruised by friction with the grains of the salt, will presently give out their juice, and produce a kind of paste of little bulk. This you will put into an earthen vessel, or small cask, and proceed in the same manner, till you have filled it. All your roseleaves being thus salted in due proportion, you will stop the vessel close, and keep it in a cool place till wanted.

Distillation.

Whenever you think proper, you may proceed to distil this fragrant paste at your leisure, putting it into a common still, and diluting it with about double its weight of pure water. Thus you will neither be hurried by the season,

* In 100 parts of sirup there are 66 of sugar, which often contains some lime. In 100 parts of the pickle there are but 25 of salt.

nor

nor inconvenienced by distance; for during the winter you may distil at Paris herbs salted long before in places the most remote from the capital.

According to some, distilled waters thus obtained are more fragrant, and at the same time yield more essential oil. Lastly I would add, that this mode of salting may be applied to some very useful purposes. For instance, if it be true, that the waters of some herbs will not keep the year round, though distilled with the utmost care; these herbs, if properly salted, will keep; so that they may be distilled when wanted, and their water employed while in full possession of its medical virtues.

Applicable to herbs, the water of which will not keep.

XIV.

On the Existence of Oxalic Acid in the Leaves and Stalks of the Rheum palmatum, or true Rhubarb; by Mr. B. L*.

IT is well known, that Scheele observed the existence of oxalate of lime in the root of rhubarb; but I am not acquainted with any person's having examined the juice, stalks, and leaves, of the rheum palmatum. Mr. Vogel and myself, surprised at the quantity of free acid in this plant, endeavoured to ascertain its nature.

Oxalate of lime in rhubarb root.

We first pounded the leaves and stalks in a wooden mortar, expressed the juice, and filtered it.

Juice of the leaves & stalks

It was clear, with a slight yellowish tinge. Its smell was weak, and somewhat like that of melilot. Its taste was evidently acid, and it reddened the paper and infusion of litmus very powerfully.

Its properties.

The pure alkalis and their carbonates changed its colour to a deep brown, without occasioning any precipitate.

Examined with different reagents.

The oxalate of ammonia produced no change in it.

With limewater a whitish precipitate was formed, insoluble in water, but soluble in acids.

The muriate of lime likewise occasions a very copious white precipitate, insoluble in water, but soluble in nitric acid.

* Annales de Chimie, vol. LXVII, p. 91.

The

The precipitate produced in it by acetate of lead is of a yellowish white.

That by nitrate of mercury is white, and in a great measure soluble in nitric acid.

Nitrate of silver produces the same phenomena.

Muriate of tin gives a yellow precipitate with it.

Contains a free acid analogous to the oxalic.

These preliminary trials show: 1, that a free acid exists in the plant: 2, that this acid is analogous to the oxalic, which is demonstrated by the precipitates obtained with limewater and muriate of lime: 3, that the oxides of lead and tin seize on a colouring extractive matter: 4, that this juice holds no calcareous salt in solution, since no precipitate is produced in it either by the pure alkalis, or their carbonates, or oxalate of ammonia.

The juice distilled.

Desirous of ascertaining whether any volatile acid existed in this liquor, we distilled the filtered juice in a retort on a sand-heat. What came over was a perfectly clear water with a slight aromatic smell, not acid, and yielding but a slight precipitate with acetate of lead, which precipitate was soluble in nitric acid. We perceived no effect from limewater, barytes-water, or muriate of tin.

No acid came over.

Residuum contained superoxalate of potash,

What remained in the retort was evaporated slowly till it had acquired nearly the consistence of a sirup. At the expiration of four and twenty hours a pretty large quantity of small crystals were found at the bottom of the saucer, which, when separated and washed, exhibited all the characteristics of acidulous oxalate of potash.

and some acid,

The supernatant fluid was highcoloured, and yielded no more crystals by evaporation. It contained scarcely any more oxalic acid, for it formed no sensible precipitate either with limewater or muriate of lime, yet it was very sour.

probably the acetous.

Though the distillation of the juice, as mentioned above, yielded no acetic acid, we are inclined to believe, that this acid exists combined, or retained by the colouring extractive matter, as is the case with the juices of a great number of plants.

The residuum dried, and treated with alcohol.

The evaporated mass reduced to the state of powder we digested in alcohol at 40. The alcohol acquired a light yellow tinge, and was acid; but the acid was not the oxalic. Indeed

Indeed it is well known, that the acidulous oxalate of potash is scarcely soluble in this menstruum.

The residuum that was insoluble in alcohol contained acidulous oxalate of potash. Being calcined and incinerated in a platina crucible, a very alkaline, white, melted mass was obtained. Its concentrated solution changed the acid sulphate of alumine into alum. This substance therefore was nothing but nearly pure potash, a little carbonated, and containing a very small quantity of sulphate and muriate of potash.

Potash found in it.

From this examination of the juice of the stalks and leaves of the rheum palmatum, or true rhubarb, it follows, that it contains, 1, a pretty large quantity of acidulous oxalate of potash: 2, an uncrystallizable acid combined with the colouring extractive matter, which is analogous to the acetic acid, and in this state exhibits some of the properties ascribed to the acid termed malic: 3, and lastly, that the presence of this acid confirms in some measure the experiments of the celebrated Scheele. We need not be at all surprised to find oxalate of lime in the root of rhubarb, since an acidulous oxalate of potash is obtained from its leaves.

Results.

Rhubarb is not the only root, that contains oxalate of lime, for Scheele found it in several others; but as this salt exists in them in very small quantity, he employed a particular process to separate it, which we shall insert here, to save trouble to those who would institute similar inquiries.

Various roots contain oxalate of lime.

Cut and bruise the roots, and pour on them muriatic acid diluted with a great deal of water. Leave them to digest a few hours; then filter the solution, and saturate it with ammonia. If the roots contain any oxalate of lime, this salt will be dissolved by the acid, and precipitated by the alkali.

Method of detecting it.

The following substances afforded this chemist more or less oxalate of lime. 1, the roots of alkanet, parsley, carline thistle, turmeric, white dittany, fennel, red gentian, swallowwort, patience dock or rhubarb, liquorice, mandrake, restharrow, Florentine orris, soapwort, squill, tormentil, valerian, zedoary, and ginger; and 2, the barks of cascarilla, cinnamon, elder, and simarouba.

Roots in which it has been found,

and barks.

XV.

XV.

Analysis of the Aplome: by Mr. Laugier*.

Found on the banks of the Lena.

Mr. Haüy has given the name of aplome to a stony substance found in Siberia on the banks of the Lena. We are indebted for a knowledge of the place whence it is brought to Mr. Weiss, who has parted with his collection to government. The aplome has some resemblance to the garnet and the idocrase, or brown hyacinth of volcanoes. It differs from the former in its specific gravity, which is 3·444, though it crystallizes like the dodecaedral garnet; and from the latter in its primitive form. The figure of the crystals of the aplome seems to indicate, that they are the result of a decrement by a single row on each edge of a cube. "This decrement," says Mr. Haüy, "is so simple and elementary, that I had selected it for the first in explaining my theory of the structure of crystals." This induced him to give it the name of *aplome*, or simplicity.

Has some resemblance to garnet and idocrase.

Loss by calcination.

Treated with potash and muriatic acid.

The aplome loses by calcination two per cent. A hundred parts of aplome reduced to fine powder were treated with four hundred of caustic potash. A red heat kept up for half an hour produced only a pasty fusion. The mass when cold was of a bottle green colour. The muriatic acid completely dissolved it. This solution, treated in the usual way, gave for every hundred parts of alumine

Its component parts.

Silex	40
Alumine	20
Lime	14·5
Oxide of iron	14·5
Oxide of manganese	2
A mixture of silex and iron	2
Loss by calcination	2
	95
Loss	5
	100.

* Journal de Physique, vol. LXVII, p. 392. Abridged from the Annales de Musée.

This

This analysis shows, that the aplome cannot be considered as any of the minerals yet known.

Under these circumstances it seems to me, when the results of analysis are not sufficiently decisive to settle the point, it is indispensably necessary to combine with its results the geometrical and physical properties of the mineral; and if we take this course here, which is unquestionably admissible, we shall be led to consider the aplome as forming a distinct species.

A new species.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

AT the meeting of this Society on Saturday the 13th of january last, the Rev. Dr. Macknight read a Mineralogical acccount of Ben Ledi, and the environs of Loch Katterin. The description of the rocks in that district (which consist of mica-slate, and clay-slate, with an overlying conglomerate, formed at a lower level from the debris of primitive mountains) tended, in the author's opinion, to illustrate one branch of the Wernerian doctrine, respecting the order of formations in the mineral kingdom. It also appeared, in confirmation of another principle in the Geognosy, that the direction from SW to NE of the strata composing the Highland mountains, corresponds to what has been observed *in general* relative to the bearings of the primitive strata in the crust of the Earth. Such a uniformity of direction, it would seem, could have resulted only from the action of powers in nature that are slow and regular in their operation; and must be referred to some original law, which, later discoveries render it probable, will be found to depend on the constitution of the terraqueous globe with regard to magnetism and electricity.

Mineralogy of Ben Ledi.

At the same meeting, the Secretary laid before the Society a communication from Mr. Scoresby junior of Whitby, comprising a meteorological journal of three Greenland voyages; with remarks on the effects of the weather on the barometer in Greenland; and on the different crystallizations of snow to be observed in high latitudes.

Meteorology of Greenland.

Mr. Singer, of the scientific institution, Princes street, Cavendish square, intends to commence a course of fourteen lectures on electricity, introductory to his course on electro-chemical science, on the 6th of march.

Lectures on electricity.

ERRATUM.

Vol. XXIV, p. 374, line 26. *For* nitrogen *read* hidrogen.

METEOROLOGICAL JOURNAL,

For FEBRUARY, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

JAN. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
28	31°	31°5	32°5	30°	30·23	Cloudy	Cloudy
29	31	33·5	34	31·5	30·27	Ditto	Ditto
30	33	34·5	34	30	30·44	Ditto	Ditto
31	33·5	44·5	45	42·5	30·35	Rain	Rain
FEB.							
1	47	48	49	45	30·17	Rain	Cloudy
2	46	46	47	42	30·09	Ditto	Rain
3	44	45·5	46	41	29·80	Ditto	Cloudy *
4	42·5	38	43	33	29·90	Ditto	Fair
5	36	41	44	33	30·08	Cloudy	Ditto
6	40·5	45	46	43	30·04	Ditto	Ditto
7	46·5	46	49	43	29·95	Ditto	Cloudy
8	45·5	47	48	41	29·97	Rain	Rain
9	44	47	47·5	46	29·80	Cloudy	Cloudy
10	48	46	49·5	40	29·78	Rain	Foggy
11	42	41	43·5	38	29·87	Ditto	Rain
12	41	40	42	36·5	29·35	Ditto	Cloudy
13	38	39	42·5	34	29·05	Fair	Fair
14	36·5	37·5	40	33·5	29·34	Foggy	Foggy
15	36	35	39	27	29·70	Cloudy	Fair
16	30	36·5	37.5	27·5	30·05	Fair	Cloudy †
17	30	32·5	35·5	25	30·10	Ditto	Fair
18	29	34·5	34·5	27·5	30·17	Snow	Rain
19	30	31·5	35	26	30·00	Fair	Fair
20	30	27	32·5	20	30·20	Snow	Ditto
21	22·5	30·5	31·5	25	30·43	Fair	Ditto
22	30	30·5	32·5	32.5	30·28	Ditto	Ditto
23	36	41	43	38	29·05	Rain	Cloudy
24	42.5	47	49	47	29·55	Cloudy	Ditto
25	48	49·5	41	36	29·46	Fair ‡	Fair
26	41	46	49	46 ‖	29·89	Ditto	Cloudy §

* Rain at 11.
† Snow at 11.
‡ Generally, the day windy, rain about 2 P M. with a little hail.
§ Rain at 11.
‖ The minimum of the Thermometer at 9, gradually increasing all the night.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL, 1810.

A mineralogical Outline of the District containing the Aluminous Schistus, in the County of York, with the entire Process practised in the Manufactory of Alum: to which is added an Analysis of the Sulphate of Alumine, and the Supersulphate of Alumine and Potash, with practical Observations and Remarks: communicated by Mr. RICHARD WINTER.

THE stratum of aluminous schistus bordering upon the sea, presents cliffs that are in general precipitous. Their height is from 100 to 750 feet.

Strata.

The sea has made, and is continually making considerable incroachments upon the land, particularly to the southward of Whitby. The abbey, built in the year 656, was situate near one mile from the sea; at present the distance is not 200 yards.

Sea encroaching on them.

This wasting of the cliffs is principally occasioned by the decomposition of the schistus from exposure to the atmosphere, and the subsequent action of the wind, rain, and breakers of the ocean. In walking under the perpendicular parts of the cliffs considerable danger is to be apprehended from the fragments of stones &c. continually falling down. Several instances have occurred of a very unfortunate nature,

Owing to decomposition.

nature, from a want of precaution, and of a knowledge of these circumstances.

The stratum 29 miles wide Direction E. & W.

The stratum of aluminous schistus is about 29 miles in width, extending from 10 miles to the southward of Whitby to 19 miles to the northward. The general direction of the stratum is from east to west, as may be inferred from its being found in the interior of Yorkshire and Lancashire.

The sea coast most eligible for alum works.

Those places immediately upon the coast are, however, the most eligible for the erection of alum works, as they possess advantages, which it is absolutely necessary the manufacturer should embrace. The immense quantity of refuse schistus and rubbish (as the covering strata of the aluminous schistus are called) to be removed, renders it requisite to erect the works in such situations as to be able to get rid of these substances with the most expedition and the least expense. The charges for draught work is materially diminished, as the coals are brought by sea from the ports of Sunderland or Shields, and delivered at the manufactory: but in those works situate in the interior, they lie under a considerable expenditure for carriage, from which the other is exempt; so that we need not be surprised at the gradual reduction of the works in the interior from seven or eight to only one remaining.

Of the Strata reposing upon the Aluminous Schistus.

Covering strata

The strata, which are generally found covering the schistus, are alluvial soil, sandstone, ironstone, shale, and clay.

Minerals of the same species, as are found in the superincumbent strata, may be collected with the greatest facility upon the seashore with additional fossils cast ashore by the waves, after having been brought down by the rivulets, or fallen from the cliffs, and afterward washed by the action of the tide *.

Stones contained in them and found on the shore, with

The variety of agates, and fossil wood converted into agate, are numerous, and equally beautiful with those brought from Germany, and admit of as high a polish.

The cornelian, mocha, onyx, opal, and chalcedony, are

* Any person, who may be desirous of obtaining any of the mineral productions of this neighbourhood, may be supplied on application to me, by letter or otherwise.

found

found in beautiful specimens, and are sought after with avidity; they are chiefly cut into seals, bracelets, rings, and other ornaments.

other substances thrown up by the waves.

Garnets are found imbedded in quartz. These occur but rarely.

A variety of flints resembling the Egyptian pebble are very common; these, when broken, present a number of concentric lines, resembling the different yearly growths in timber; and variously coloured. Sometimes fossil echini are found enclosed in the flints, others contain cubical pyrites.

Jaspers of a red and green colour, elegantly variegated, occur in abundance.

Some of the nodules of quartz contain very fine small rock crystals, regularly formed.

Mica is sometimes to be met with in the sandstone strata.

Two species of hematites, or bloodstone, red and green. The green is very scarce.

Pieces of amber are sometimes thrown ashore by the sea.

Coral is also found but of a very inferior kind.

The os sepiæ, or bone of the cuttle fish, is common, the fish itself is often cast ashore.

The madreporæ are frequently to be met with.

Puddingstone, porphyry, granite, and whinstone are found in masses of various sizes.

The metallic ores are less numerous. Iron is the most predominant.

Manganese is met with in small quantity, in the state of a black oxide.

Lead has been found crystallized, but the specimens are rare.

Sandstone strata.

The sandstone strata are found of various qualities, seldom very white, generally of a brownish red: sometimes the quantity of ferruginous matter contained in them is so considerable, as to make them very hard. Even the softer kind of sandstone, by exposure to the atmosphere, is found to grow much harder, so that it is very useful in building. The thickness of the strata varies from four yards to upwards of fifty: the general inclination or dip is to the S. W.

Springs.

Immediately under the sandstone strata are to be found

springs of very good water, some of them highly impregnated with iron, united with carbonic acid gas; the acid becomes disengaged by exposure to the air, and a copious precipitate of the iron, of a dull red colour is deposited. The temperature of several of these springs is from 44° to 46° Fah. uniformly throughout the year.

Coal and jet.

Under the sandstone is frequently found a seam of coal, or jet, of an indifferent quality. Sometimes these occur enclosed in the substance of the sandstone, but they rarely exceed two inches in thickness, and are of no great extent.

Clay.

The clay is chiefly alluvial, or derived from the decomposition of shale. The colour is generally of a bluish gray, sometimes of a yellow ochrey colour. The thickness of the strata may be averaged at two feet.

Ironstone.

The ironstone is found in loose or broken strata, from two to twelve feet thick; the quality is much inferior to those seams of iron ore found in the aluminous schistus, noticed hereafter. Its specific gravity is about 3·1. It may here be remarked, that the whole of the strata are traversed by veins, intersecting each other at right angles, in a southerly and an easterly direction. The masses, both of the schistus and sandstone, always appear in the form of solid parallelograms, occasioned by the crossing of the veins, the longest side of the solid lying between N. and S. I have noticed, that, when the stratum of clay has been uncovered for a considerable time in the summer season, on the abstraction of water from the clay it cracked in regular divisions, of the same rectangular figure as those visible in the sandstone and schistus. This observation on the regularity of the divisions assumed by alumine in drying is noticed by Chaptal (Chemistry applied to the Arts, vol II, p. 46). Perhaps the formation of the basaltic pillars may have been effected by this combination of fire and water in some gradual manner.

Of the Mineral and Fossil Bodies found in the Aluminous Schistus.

Fossils found in the schistus

A very pure native alumine is found enclosed in a nodule of stone resembling indurated clay. Several species of ammonitæ are found enclosed in an argillaceous ironstone of

of a double convex form. Two or three species of nautili occur, these last are rarely found in a perfect state. The belemnitæ are very abundant. The trochitæ are found, but not in great abundance. The fossil vertebræ and other bones of animals are frequently found, the form of which has been but little deranged. I found a part of the os femoris of an animal with the trochanter and the foramina very evident; the part where it was broken measured 4·2 inches in diameter, so that its length when in a perfect state may be inferred to have been at least 4 feet. The shells are numerous, of various species, and some of these are in a state of great preservation.

Naphtha is sometimes found enclosed in an ironstone of a globular form. Jet is found in abundance, frequently the bituminization is not perfect, and one part of the substance presents us with pure jet, while the other is still in the state of petrified wood: in this state it is most commonly found, in breaking up large masses of iron ore.

There is an immense quantity of red iron ore, found in strata, at the depth of about 200 feet from the top of the aluminous strata: the thickness of these seams of ore vary from about a few inches, to about 2 feet. In some situations four or five of these strata are found alternating with schistus. The specific gravity of this ore is from 3·4 to 4·2. It contains, upon analysis, from 30 to 60 per cent of iron, combined with oxigen, phosphoric acid, lime, alumine, and silex. Considerable quantities of this ore are collected, and carried down to Newcastle, and smelted at the founderies erected there for this purpose.

Sulphate of lime is found crystallized in radiated and striated crystals; but this is at considerable depths in the rocks.

Carbonate of lime is generally found crystallized, filling the veins which intersect the aluminous schistus. The thickness of these veins of crystallized carbonate of lime is generally 0·5 of an inch, and they are of considerable depth.

New variety of alum rock.

An ingenious landscape painter, and a good mineralogist, Mr. Bird, of this place, has recently discovered a new variety of alum rock, containing silex and sulphur, with oxide of iron. This rock effloresces on exposure to the atmosphere, and

and a sulphate of alumine is produced. The stratum is of great extent, and inestimable value. I am not permitted to point out its situation.

Aluminous schistus.

The aluminous schistus is generally found disposed in horizontal laminæ. Sometimes it exists in the form and appearance of indurated clay; in fact the whole of the upper part of the stratum resembles indurated clay, when first wrought; but by exposure to the atmosphere it suffers decomposition, and crumbles into thin layers. The upper part of the rock is the most abundant in sulphur, and the deeper they work into it, the quantity of sulphur decreases, and the bituminous substance increases, and the rock becomes more hard and slaty; so that a cubic yard of rock, taken from the top of the stratum, is as valuable as 5 cubic yards taken at the depth of 100 feet.

When a quantity of the schistus is laid in a heap, moistened with sea water, it will take fire spontaneously, and will continue to burn until the whole of the combustible materials are exhausted.

A considerable part of the cliff some years ago fell down in a situation where it was exposed to the sea at high water; in a short time afterward combustion had taken place throughout the whole extent of this small volcano, and it continued to burn for two or three years before it became extinct. Does not this fact explain the nature and cause of volcanoes? This point I am aware has been ably illustrated in the spontaneous inflammation of pyrites, the artificial volcano of Lemery, and more particularly by the indefatigable Spallanzani, and Sir W. Hamilton.

Volcanoes.

The whole extent of the aluminous strata bears evident marks of a volcanic nature. It is intersected by whin dykes, and wherever the coal strata come in contact with these dykes, the coal is charred to some distance. Wood is also found in every part of the schistus converted into charcoal. Jet appears to be some vegetable substance, that has been acted upon by considerable pressure, and some degree of heat, not sufficient to convert it into charcoal; it frequently has the appearance of a cylinder having undergone an immense pressure, and the centre filled with pyrites. The accumulation of sulphur towards the top of the strata, as if it had been sublimed—these facts seem to countenance the

the idea, that this is a volcanic country, where the degree of heat has not been sufficient to put the rocks into a more rapid state of combustion; or for want of the access of the sea into the interior parts of the Earth. Who can determine, that nature has not yet remaining rocks, which may become volcanoes in some future ages, when the sea has found a sufficient inlet into the bowels of the Earth.

The observations I have made with regard to the chemical nature of the schistus are merely indicative of the substances contained in it. Indeed experiments would only exhibit a conjectural, and not a real analysis of the schistus, unless a considerable number of them were made at different depths, and in various situations of the stratum.

The colour of the aluminous schistus is a bluish gray. Its hardness differs; at the top part of the strata it may be crumbled in pieces between the fingers, at a considerable depth it becomes as hard as roof slate. The specific gravity is about 2·48.

Characters of the aluminous schistus.

Alcohol digested upon it, and afterward evaporated, leaves a residuum having all the properties of petroleum.

Olive oil, digested upon the schistus, acquired a dark brown colour, most probably from bitumen.

Exposed to a red heat for a considerable time it loses 15 per cent, and assumes a whitish colour, if taken from the top of the rock, and a dull red colour, if taken at about the depth of 40 yards.

Dilute sulphuric acid was poured upon a portion of the schistus; and upon adding prussiate of potash, an abundant, precipitate of prussiate of iron was thrown down from this solution.

Ammonia precipitates a very considerable proportion of alumine, amounting to 30 per cent, in some instances.

Oxalic acid discovers the presence of lime and magnesia.

Fused with an alkali, muriatic acid precipitates a large proportion of silex.

Hence the aluminous schistus contains silex, alumine, magnesia, lime, oxide of iron, bitumen, sulphur, and water.

Of the Calcination and Lixiviation of the Schistus.

The covering strata are removed previous to working the

Method of working it.

Method of working the schistus.

the alum rock (as it is generally called). The hewing of the rock is performed with picks and javelins; and it is conveyed to the calcining place in barrows, so contrived, that the centre of gravity of the weight, is in a perpendicular line passing through the centre of the axle of the wheel; by this means the men have nothing more to do, than to keep the barrow steady, throw the weight of the substance upon the wheel, by raising the handles, and direct the barrow upon the way, which is formed of cast iron plates, 6 feet in length, 6 inches in breadth, and half an inch thick; these plates are fastened into cross pieces of wood fixed into the ground, at the end of each plate. Ten of these barrows contain one solid yard of the rock. The expenses of working the rock vary according to the facility with which it can be hewn. When the distance the rock is to be barrowed is about 200 yards, the rate for removing and hewing one cubic yard is about 6½d. It is unnecessary to state, that the price must maintain a corresponding ratio with the distance to be conveyed. The men earn about 2s. 6d. per day in the winter season, and 3s. in the summer.

The rock is poured out of the barrows upon a bed of fuel, composed of underwood, furze, &c. The dimensions of this pile of faggots is about four or five yards in breadth, and two in height; as the rock is deposited upon the fuel, it is necessary that it should be broken into small fragments, that the combustion may take place with the greater facility. When they have got about four feet in height of the rock upon the faggots, fire is set to the bottom, and fresh rock continually poured upon the pile; other piles of wood are then placed alongside of the first, and they proceed as before, adding more rock, firing the fuel, &c. This they continue, until the calcined heap is raised to the height of 90 or 100 feet, and from 150 to 200 feet in length and breadth. Some of these heaps of calcined mine (as it is now called) will contain 100,000 solid yards of schistus or rock.

When the whole heap is in a state of combustion, a considerable quantity of sulphureous acid gas is disengaged, this they endeavour to prevent, by moistening small schistus, and forming a kind of clay; with this they plaster the outside

side of the heap, this however does not prevent the escape of the gas in any degree, but it prevents the wind from penetrating, and assists in preventing the calcined mine from falling, by forming a kind of crust all over the heap; this crust is soon decomposed by the action of rain, &c.

The form of the places for calcining the rock in is badly calculated to prevent the escape of the sulphureous acid gas. If the combustion was effected in a building of the shape of a smelting furnace, immediately upon the whole of the rock becoming ignited the openings might be closed, and the gas preserved. I have ascertained by experiment, that nearly one half of the sulphureous acid gas is expelled by a red heat, continued for a considerable space of time.

Every suggested improvement is considered as an innovation by the illiterate, and it may be truly said, to be more easy to remove mountains than long established prejudices; the anxious manufacturer is seldom sufficiently master of his works, so as to be able to turn the scale of long established custom: and the most enlightened and scientific methods are entirely defeated, when trusted to the hands of workmen to carry them into execution.

Difficulty of introducing improvements.

How little melioration can be expected among a class of people, where reason has never made any impression upon the mind! I would hail the man as a true patriot, who shall endeavour to disperse this cloud of darkness from the human race.

The sulphureous acid gas, by absorbing oxigen from the atmosphere, is converted into sulphuric acid; this change is effected by means of the oxide of iron contained in the mine, and moisture. It would certainly be worth ascertaining by experiment, whether the oxide of iron combined with sulphur in burning would not yield sulphuric acid if moistened with water.

I am aware that iron has a greater affinity for oxigen than sulphur has in the fire, but in the great scale of nature she observes laws peculiar to herself: the affinities observed in the salts of the ocean are contrary to the order they appear in our tables, here, we find the small portion of sulphuric acid united to the lime; instead of forming a union with the soda, as might be inferred. Lime is found to decompose

pose the muriate of soda. These, and other anomalies might be produced, but they are foreign to the purpose.

Making the alum.

130 tons of calcined mine will produce 1 ton of alum. I have deduced this number from an average of 150000 tons of calcined mine consumed.

The calcined mine is steeped in water, contained in pits, that usually hold about 60 cubic yards. The water thus impregnated with sulphate of alumine, called alum liquor, is drawn off into cisterns, and afterward pumped up again upon fresh calcined mine. This is repeated until the liquor becomes concentrated to the specific gravity of 1·15; or 12 pennyweights of the alum maker's weight. The half exhausted mine is then covered with water, successively, to take up the whole of the sulphate of alumine; these liquors, thus impregnated, are denominated strong liquor, seconds, and thirds.

The strong liquor is drawn off into cisterns, to deposit the sulphate of lime, iron, and earth suspended in it. In order to free the liquor from these substances, they clarify it by boiling for a short time, which enables the sulphuric acid to exert its affinities with greater energy. After running it from the pans, and suffering it to cool, the whole of the sulphate of lime, iron, superfluous alumine, and earth, are deposited; and the alum liquor is nearly pure. Where this precaution is used, the alum is much better in quality, and almost entirely divested of the sulphate of iron. This method is only practised at some of the works, owing to the additional quantity of fuel required, and consequently increased expense.

The liquor in this state is carried by means of pipes, or wooden gutters, into leaden pans. These pans are made of sheet lead (cast by the workmen in the alum house) 10 feet long, 4 feet 9 inches wide, 2 feet 2 inches deep at the hinder part, and 2 feet 8 inches at the front end: this difference is allowed to give a rapid current in running off.

A quantity of mothers is pumped into the pans every morning; and, as this evaporates, the deficiency is supplied with fresh alum liquor, every two hours, or, as the liquor in the pans becomes more concentrated, the additions are made more frequently. It is necessary to keep the pans

continually

continually boiling, otherwise the superfluous alumine and sulphate of alumine, deprived of its water of crystallization, would be precipitated, and the pans melted, from the crust formed between the liquid and the lead.

Each pan will produce upon an average 4 cwt. of alum daily, and the consumption of coals will be about 18 bushels Winchester measure.

The liquid contained in the whole of the pans is run off every morning into a vessel called a settler, at the same time a quantity of alkaline lee is brought along with the boiling liquor, prepared either from kelp, soapers lees, (generally called black ashes) or muriate of potash, of a specific gravity from 1·037, to 1·075. The alum maker having previously ascertained the specific gravity of the liquid in his pans, estimates the quantity of alkaline lees to be added, necessary to reduce the liquor from the pans from the specific gravity of sometimes 1·45 or 1·5 to 1·35.

The liquor then stands in the settler about two hours, that it may deposit the sediment it may contain, when it is run off into the vessels (or coolers) to crystallize.

If the alum maker should be below, or equal to the specific gravity of 1·35, in mixing the alkaline lee and liquor, there is nothing more to be done. If he exceed this specific gravity, he then adds urine in the coolers, until the liquid is reduced to 1·35. It is then agitated to combine the heavy and light liquids, and then left to crystallize. It must be observed, that at a greater specific gravity than about 1·35, the liquor, instead of crystallizing, would present us with a solid magma resembling grease.

After standing four days, the mothers are drained off, to be pumped into the pans again the succeeding day. The crystals of alum are conveyed into a tub, where they are washed in water, and put into a bin, with holes in the bottom, to allow of the water draining off from the alum. They are then removed into a pan (twice as large as the common leaden pans), and as much water added as is found requisite to dissolve the whole of the alum when in a boiling state: the moment this is effected, the saturated boiling solution is run off into casks. These casks should stand about 16 days; as they require this time to become perfectly

fectly cool, in the summer season. The casks are then taken to pieces, and a hollow cask of alum is produced; it is then broke into, and the whole of the saturated solution of alum (called tun water) is removed back into the pans, to go through the process anew.

This last process is called roching. The outside of the cask of alum is now to be cleared from dirt, and the sediment which is deposited at the bottom. It is then broken up into masses ready for the market.

Practical observations and remarks upon the foregoing processes.

Method of ascertaining the specific gravity.

The method pursued by the alum makers to find the specific gravity of any liquid is capable of considerable accuracy. A bottle is procured, that will contain about ½ of a pint. The narrower the neck, the more accurate will be the results obtained by it. This bottle is balanced in a pair of sensible scales, we will suppose it to weigh 1000 grains, it is then filled with distilled water, and carefully dried with a cloth; now allowing the water to weigh 2400 grains, this last number is divided into 80 parts or pennyweights, and we have 30 grains corresponding to one pennyweight; this they subdivide into ½ and ¼. Hence we may ascertain the relative specific gravity of any liquid. 1 pennyweight is equivalent to 1·0125, and 80 pennyweights to 2·0. Care however is necessary, to have a counterweight of 3400 grains, equal in weight to the water and bottle together, which must always be put into the scale, along with the other weights, in operating. This was formerly a great secret among the alum makers, and they sold the method at a high price, or handed it down to their children as an hereditary possession.

Improvements suggested.

Considerable advantage might be derived to the manufacturer, by reducing the size of the fire places, and erecting iron doors, to prevent a current of air passing over the fire, instead of entering by the ash pit: a very material saving of fuel would arise from adopting this method.

A very material errour is committed, by concentrating the liquor in the pans to near the specific gravity of 1·5, and

and then reducing it again to 1·35: this method obliges them to evaporate a very unnecessary quantity of water.

The alum liquor is frequently brought into the pans as low as 1·09; when by repeatedly bringing the liquor over fresh calcined mine, it might be concentrated to 1·25, or more. I will mention an instance where the expenditure in evaporating liquor was more than £3 10*s.* daily; when at the same time this liquor might have been concentrated to an equal degree, by repeatedly pumping the liquor upon fresh calcined mine, at an expense of not more than 9*s.* in the same time; here there was a loss of £3 1*s.* daily.

In using black ashes, or kelp, a considerable quantity of charcoal is dissolved in the alkaline lee; this charcoal is precipitated on adding a small quantity of the solution of sulphate of alumine, but is redissolved again by adding the solution in excess.

This charcoal then contaminates the alum, and decomposes a quantity of the sulphuric acid: therefore, it must appear conclusive, that whatever alum is made with muriate of potash alone will be far superior in quality, while the produce will be greater in quantity.

It might be supposed, that urine was a necessary ingredient in the making of alum; but the fact is, it merely hides the ignorance of an alum maker. Having no determinate rule to guide him, in reducing the liquor from the pans, should he chance to exceed the specific gravity of 1·35, he adds urine, or some such light fluid, to bring the liquor as near as possible to this density. The alum works, that approach the nearest to the true chemical principles, are those of the Right Hon. Lord Dundas, and Messrs. Baker and Co. They use no urine in these works—the alum liquor is always clarified previous to its being used—they use no alkali generally, but crystallized muriate of potash—greater economy is observed in the consumption of fuel; and the result is a product of alum considerably larger in a given time, and of better quality, than can be produced by the works established upon the old plan.

Remarks on the alkalis employed in the manufacture.

The kelp used is obtained by burning the sea wrack in kilns, at a great number of places upon the coast of England, Scotland, &c. It is a very inferior alkali in an alum manufactory.

manufactory. It contains about 47 of soluble salts, and 53 of charcoal, sand, and earth. The salts are muriate of soda, soda, and sulphate of soda.

The refuse of the soap boilers' lees are burnt in a kind of oven, and sold under the name of black ashes. The composition of these ashes is about 90 of soluble salts, and 10 of charcoal and earth, the salts contain muriates of soda and potash, sulphate of potash, and muriates of lime and magnesia.

I have always found great difficulty in producing alum by the muriate of soda, and never could form alum in any way by means of pure soda.

The muriate and sulphate of potash are the only alkalis that can be used to advantage in the composition of alum.

I have made comparative experiments to ascertain the quantity of the different alkalis it would require to produce 100 tuns of alum. The following are the results:

22 tuns of muriate of potash will produce 100 tuns of alum,
31 ditto of black ashes .... 100 ditto,
73 ditto of kelp .... 100 ditto.

The alkalis are considered as in the state, in which they are found in commerce.

Analysis of sulphate of alumine, and supersulphate of alumine and potash.

Analysis of sulphate of alumine.

I have been generally disappointed in analyzing alum on finding my results at variance with those of so many eminent chemists. It appears, that the errour has existed in their different estimations of the composition of sulphate of barytes. It seems, that allowing about 33 per cent of acid is very near the truth*. By taking it in this ratio, the acid

* According to some very careful experiments made by Mr. Arthur Aikin, see Journal, vol. XXII, p. 301, it is nearer 34. He makes it 33·96; and according to Klaproth it is 33·55. See also an Analysis by Mr. James Thomson, vol. XXIII, 174; and another by Berthier, ib. p. 280: both of whom make it at least 33. C.

used,

used, and the quantity of alum produced upon a large scale nearly correspond.

I know of no experiments, that have been made to ascertain the composition of sulphate of alumine, except Bergman's. I believe Vauquelin has done the same, but as I have not seen his paper, I cannot speak to that effect. Bergman states the composition

Sulphuric acid	50
Alumine	50
	100

From a solution of pure sulphate of alumine in water I precipitated the acid, by means of the muriate of barytes added in excess. The precipitate, after having been carefully washed upon a filter, was exposed to a white heat for some time. The quantity of acid inferred from the sulphate of barytes obtained, was 287 grains.

To an equal quantity of a solution of sulphate of alumine, ammonia was added to saturation; the precipitate, after washing upon a filter, was exposed to a white heat for an hour, and weighed 209 grains. Therefore sulphate of alumine is composed of

Sulphuric acid	57·8
Alumine	42·2
	100·0

Analysis of alum.

Analysis of supersulphate of alumine and potash.

Exp. 1. To 10000 grains of a very pure crystal of alum, dissolved in rain water, was added muriate of barytes, until no farther precipitate took place. The sulphate of barytes, after having been well washed upon a filter, was exposed to a white heat for some time, it weighed 3359 grains.

Exp. 2. To 10000 grains (of a part of the same crystal, as used in the preceding experiment) in solution, was added ammonia, until the precipitation was found to be complete. After washing and filtering the alumine, it was exposed to the heat of a blast furnace, and found to weigh 1096 grains.

Exp.

Exp. 3. The remaining solution of the 2d experiment, after boiling quicklime in it to take the acid from the ammonia, was evaporated to dryness, and afterward exposed to a red heat. The potash, or soda, thus produced, weighed 858 grains.

I repeated the analysis twice, the results, after taking every precaution, are as under:

	1st Exp.	2d Exp.	Mean of the two.
Sulphuric acid	33·59	33·10	33·34
Alumine	10·96	11·81	11·38
Soda or potash	8·58	9·74	9·16
Water	46·87	45·35	46·12
	100	100	100.

In finding the quantity of water contained in crystallized alum, by exposure to a red heat, an uncertain product will always be given, arising from the degree of heat employed in the desiccation of the alum. At a white heat a very considerable proportion of acid will be expelled, as well as the water.

In the production of alkali from the alum, I have called it soda, or potash. I did not institute any experiments to ascertain the quality of the alkali, but as nothing but kelp and black ashes had been used in the fabrication of the alum, it is evident the alkali must have been soda with perhaps a small proportion of potash. So that in reality we have a fifth variety in addition to the four described by Dr. Thomson.

It remains then of some importance to the consumer to ascertain the difference between alum made with potash, and that in which a salt of soda is used.

I have received considerable advantage from the very able memoir of Messrs. Thenard and Roard, as inserted in your Journal, vol. 18, page 276.

Had these philosophers adopted 33 per cent of acid in the sulphate of barytes, instead of 26, as is stated in their paper, our results would not have been very different. By correcting their statement of the composition of alum, according

cording to the estimate of sulphate of barytes, which I have used, we shall find it to be

Sulphuric acid	33·05
Alumine	12·53
Potash	7·90
Water	46·52
	100.

Whitby, 10*th March*, 1810.

II.

Tools to answer the Purpose of Files and other Instruments, for various Uses, made of Stone-ware. By G. CUMBERLAND, *Esq.*

To Mr. NICHOLSON.

SIR,

TO some men, but not to you, will it appear a trifle, because very obvious on reflection, to have applied so soft a substance as clay to the purpose of lograting the hardest bodies; neither should I perhaps have ever thought of such an application in the form I now use it, had I not found, in shaping some substances, that the wear of my steel files was rather expensive.

Clay employed to abrade hard bodies.

It then first occurred to me, in ranging in thought after a remedy, that, as our stone-ware is so hard as to blunt our files, files might be as well made of our stone ware. This was about two years ago, and the first use I made of the suggestion was, to fold up in muslin, cambrick, and Irish linen, separate pieces of wet clay, forcing them by the pressure of the hand into the interstices of the threads, so as on divesting them of the covering to receive a correct mould. These I had well baked, and immediately found I had procured an intire new species of file, capable even of destroying steel; and very useful indeed in cutting glass, polishing, and rasping wood, ivory, and all sorts of metals.

Tools made of it for this purpose.

The ease with which I had accomplished my purpose, as is too often the case, made me content myself with the use of

Uses to which they may be applied.

of my own discovery, or at most giving away a few specimens as files for ladies nails of peculiar delicacy: but having since reflected, that in glass grinding (the stones for which come from the North, and are very expensive) in flatting metallic mirrors, laying mezzotinto grounds, and a number of operations that require unexpensive friction, these stone-ware graters, if I may so call them, as not being of the exact shape of files, may ultimately become very useful. I take a pleasure in furnishing you with a description of my method of applying this substance, accompanied with a specimen or two of a portable size, that you may the better be able to judge of their value to the arts, which to me, the more I reflect on them seem the more important; as in all operations of grinding a great deal of manual labour must first be bestowed on the tool, whereas here we may mould ours in an instant, if we use a press, as in pipe making, and the expense is infinitely inferior to that incurred in constructing even the cheapest file or logrator.

Some of their advantages.

I am, Sir,

Your most obedient humble servant,

Bristol, Feb. 10th, 1809. G. CUMBERLAND.

Perhaps they might be usefully employed for speculums.

P. S. I have not yet tried it, not having the means just now at hand, but if a good parabolic reflector were to be impressed with a mass of stone-ware clay covered with muslin, so as to make several casts of different degrees of fineness, we might this way acquire tools, that would greatly lessen the expense of the operation of grinding; but much would depend on care in baking. Our stone ware warps but little ever.

ANNOTATION.

This ingenious invention promises to be of considerable use in the arts. The abrasion of surfaces is performed either by a toothed tool, as in filing, rasping, &c.; or by a grinder, in which cutting or hard particles are bedded with considerable firmness in a softer mass; or by scowering, polishing,

polishing, &c., in which hard particles are more or less slightly retained in a soft or tenacious substance. Mr. Cumberland's instruments appear to promise great utility in the first and last of these processes; that is, they may be used either with or without a fretting powder. There are however many objections to their being used to grind speculums; not only with regard to the intended figure, but the nature of the material.

W. N.

III.

Remarks on Meteorology. In a Letter from THOMAS FORSTER, *Esq.*

To Mr. NICHOLSON.

SIR,

I Was much pleased with the perusal of your correspondent Dr. Bostock's letter on meteorology, p. 196, and particularly with his plan of a diary. I am of opinion, with him, that an attention to the several modifications of clouds, and the changes of weather which succeed them respectively, is of great importance in meteorology; and it is much to be wished, that more accurate and frequent observations of this sort were made. A want of a nomenclature capable of expressing clearly all the different modifications of cloud has been the reason, why this branch of the science has been hitherto so little attended to by meteorologists, but your correspondent does not seem to be aware, that this is no longer a desideratum in meteorology. The ingenious Mr. Luke Howard, of Plaistow, published a few years ago a short treatise on the various modifications of clouds, in which he has determined their distinctive characters, and shown the manner in which they are probably formed. The substance of this pamphlet may be found in Rees's Encyclopedia under the word cloud. In a meteorological diary, which I keep, I always note down the different modifications, placing in a parallel line all those which appear in the sky at once. I

Accurate observations of clouds important to meteorology.

Nomenclature of them by Mr. Howard.

Plan of a meteorological diary.

S 2 would

would recommend the adoption of the following plan for a meteorological diary. It should consist of twelve columns headed as follows. 1st column, day of the month; 2nd and 3rd the maximum and minimum of the thermometer; 4th and 5th, ditto of barometer; 6th, the quantity of rain which falls in the course of the day; 7th, the quantity of evaporation in square inches; 8th, the state of the hygrometer; (de Luc's is perhaps the best) 9th and 10th, the direction and force of the wind; 11th, the modifications of cloud; and the 12th should be reserved for the register of occasional meteorological phenomena, such, for example, as thunder storms, meteors, &c.

The insertion of these hints in your truly scientific Journal will oblige

Yours, &c.

THOMAS FORSTER.

No. 6, St. Helen's Place,
Mar. 8th, 1810.

IV.

On expectorated Matter; by George Pearson *M. D. F. R. S.*

(Concluded from Page 229.)

Sect. III. *Agency of Alcohol of Wine.*

Opaque viscous matter desiccated and digested in alcohol.

1. (a) TWO thousand five hundred grains of desiccated expectorated matter of the fifth sort, sect. I, 5, being the one twentieth of 50000 grains of matter previously to evaporation to dryness, were digested in four pints of alcohol of spirit of wine, of the specific gravity of 815, water being 1000. The mixture was exposed at a temperature from 58° to 68° for a month, during which it was frequently shaken. A tincture of the colour of red port wine was then decanted from off a blackish sediment. By means of a press, two ounces more of the tincture were obtained.

Residuum.

(b) The undissolved residuary matter being exsiccated weighed 130 grains less than before digestion. On exposure

posure to the air, it remained dry, but it became more flexible. It no longer emitted ammonia on trituration with lime.

(c). The tincture thus obtained was distilled readily till there remained about five ounces measure in the retort, and what remained seemed to be chiefly water instead of spirit, with such a quantity of matter dissolved in it, as not to afford liquid by distillation, without frequently spirting into the receiver. The residuary liquid was therefore evaporated to the consistence of a soft resinlike extract of a black colour; which had a salt with bitter taste.

The tincture distilled.

The distilled liquid had a peculiar pungent smell, but not that of ammonia, and it neither reddened turnsole paper, nor rendered violet cloth green.

Distilled liquid.

(d). The resinlike extract (c) weighed 140 grains. It was semitransparent—dissoluble in water, but not coagulable in boiling water—it grew softer on exposure to air—it was uncrystallizable—it betrayed no signs of alkalescency or of acidity, except giving turnsole paper a reddish hue—under the blowpipe it burnt like matter from animals, and afforded a fused globule, which indicated muriate of soda, and a large proportion of potash, deliquescing very speedily —with lime it emitted the smell of ammonia—with phosphoric, and also with tartaric acid, on being heated, an acid smell was perceived, which I at first mistook for acetous acid; but I soon found that no such acid was present, not being able to detect a trace of any acid in the distilled liquid from these mixtures—on the addition of acetite of lead, a very copious precipitation of fawn-coloured sediment instantly took place, with the smell most distinctly of apples. The decanted liquid of this mixture was found to be chiefly acetite of potash. On dropping diluted sulphuric acid upon the fawncoloured sediment, it constantly emitted the smell of apples. I could not, however, satisfy myself, that the small quantity of liquid decanted from off this sediment contained a kind of vegetable acid for the first time apprehended in the fluids of animals; because, first, the quantity of product I possessed was so diminished by many experiments, that I was unable to make what I considered to be decisive trials. Secondly, because in

Extract.

Supposed acid.

Perhaps mistake.

in subsequent processes I failed in producing the same apple-smelling liquid. Hence I considered, that the supposed acid, which had some of the properties of the malic, only occurred occasionally; or that I had been deceived, and that I had procured nothing more than a little of the acid employed for the decomposition, disguised by mixture with the subject of the experiments. The fawn-coloured precipitate was, no doubt, chiefly muriate of lead. Still the experiments fully demonstrated the presence of potash neutralized, either by an acid destructible by fire and dissoluble in alcohol, but hitherto not obtained from animal oxide, or that an oxide of animal matter alone neutralizes the potash, as will be manifested by the evidence of experiments to be related.

Potash and muriate of soda.

(*e*). Forty-five grains of the residue (*c*) which had been dissolved in alcohol, being burned in a platina crucible, yielded chiefly potash, and half its quantity of muriate of soda.

(*f*). Twenty-five grains of the residue (*c*) were boiled with successive portions of nitric acid, till the oxide of animal matter was decompounded; and carried off in the state of gasses; and then deflagration took place, leaving subcarbonate of potash with muriate of soda and charcoal.

Component parts of the extract.

According to a computation, the 140 grains of resinlike extract (*c*, *d*) consisted of twenty-eight grains of potash, and eighteen grains of muriate of soda, with an inappreciable quantity of ammonia, and perhaps phosphoric acid, beside the oxide of animal matter, and possibly an acid of an unknown kind.

Matter not soluble in alcohol.

(*f*). The undissolved matter (*b*) was burned in a platina crucible. It afforded a residue, which I could not render fluid by fire, but only of the consistence of paste. On cooling, it was a brittle gray mass weighing fifty-six grains, somewhat salt and gritty to the taste. It consisted of muriate of soda, and phosphate of lime, about twenty-three grains of each,—of potash four grains,—of fusible matter, which by long boiling in muriatic acid yielded phosphate of lime, muriate of lime, and utterly indissoluble vitrified matter with traces of magnesia, oxide of iron, and a sulphate.

2. Four

2. Four thousand grains of expectorated matter of the third kind, page 219, sect. II, 7, were added to two pints of rectified spirit of wine. By agitation the spirit became at first milky, but presently it grew clear; little curdy masses appearing, which fell to the bottom as a sediment, being in bulk about one fourth of that of the added expectorated matter.

Opaque ropy matter digested in alcohol

After a month's digestion, the filtrated liquid, on evaporation, afforded a dry extractlike residue, weighing sixty grains. It grew moist by exposure to the air, but not when kept in close vessels. It consisted of the same ingredients, but in very different proportions as the residue from distilling and evaporating the mixture page 221, Sect. III, 1, the present residue containing a much larger proportion of muriate of soda, and oxide of animal matter.

Extract.

Successive digestions of the same matter afforded less and less saline residue, but nearly the same proportion of oxide of animal matter for three times, but then no saline matter was afforded, but merely animal matter. The residues of the evaporated tinctures of the subsequent digestions did not, like the first, grow moist, but only softer; and the oxide of animal matter from each of them was no longer coagulable, although afforded by dissolution of coagulated matter. It appeared that the animal oxide was of one kind only, and that the whole of it might be dissolved in alcohol, and thereby become uncoagulable, and more easily dissoluble in every kind of menstruum.

Successively treated in the same manner.

Animal oxide.

3. If a large proportion, namely two parts of expectorated matter be mixed with two parts of rectified spirit of wine, the matter is in great part at least, coagulated, but the spirit is rendered milky. The same is true with regard to other menstrua. The reason is obvious. The coagulation is produced by the separation of water from the animal oxide of the expectorated matter, by the attraction of the alcohol, or of acetous acid for the water; but if there is not a due proportion of spirit or acid, the oxide of animal matter retains so much of the water, as to render the liquid milky. A person accustomed to these experiments may determine pretty exactly, by means of them, the proportion of water in the expectorated matter, it being directly

Mixture of equal parts.

Coagulation.

Proportion of water determined.

rectly as the quantity of spirit or acid requisite to produce entire coagulation in a clear liquid; and the proportion of coagulable animal oxide is, within certain limits, inversely as the quantity of spirit requisite for coagulation.

Opaque ropy matter digested in ether.

4. Sulphuric ether, being in many properties analogous to alcohol of wine, I digested three hundred grains of exsiccated matter of the third kind, page 219, in four ounces by measure of this menstruum for a month, in a warm room, during which the vessel was often agitated. Three ounces of a black tincture were thus procured, which, on distillation to dryness, afforded sixty-five grains of soft extract. This extract became a little moist on exposure to the air, and was then a little viscid. It burnt with flame like oil to the state of charcoal; and this again, on burning, only left two grains of residue, which consisted of muriate of soda, with indications of alkali, and phosphate of lime.

Residuum.

The undissolved residue also remained soft, and could not be made brittle by evaporation. After inflammation and incineration, the usual products were obtained as from matter which had not been digested. This menstruum had therefore dissolved abundantly the oxide of animal matter, and but a small proportion of the saline and earthy parts.

Apparently uniform expectorated matter not of the same consistence throughout.

4. Apparently uniform expectorated matter is not of the same consistence through the whole mass; for a few drops of the opaque kind being shaken in half a pint of rectified spirit of wine, the whole does not dissolve, but it is broken into small curdy particles, which fall as a sediment in a clear liquid, seemingly about one fourth of the original bulk of the matter.

Sect. IV. *With Water.*

2d and 4th kinds treated with cold water,

1. None of the kinds of expectorated matter are readily diffusible through cold water, except the second and fourth, page 219 and 221; and by agitating them some fibrous pieces are usually detached; also on inspecting the water after this diffusion, it appears full of small masses, or motes. On standing, these suspended masses become a sediment; which is the case, although the proportion of expectorated matter be exceedingly small to that of the water.

2. When

2. When very hot water is used, namely, that of a temperature from 190° to 210°, a still greater number of motes are perceivable, especially with a lens, and the water is rendered milky.

And very hot water.

3. Brisk agitation is required, for a due length of time, to diffuse the other kinds of expectorated matter through cold water; but a great number of fibrous and membranous pieces appear, the form of which cannot be destroyed, or only partially, by shaking, in almost any proportion of water. Three drops of ropy and opaque matter were shaken in half a pint of distilled water. About one half of them was diffused; the rest of them was in the form of small fibrous, leafy, and irregular figured motes; which, on repose, formed a sediment, and remained in that state three months; although in that time the water became highly fœtid, and sometimes in this experiment the sides of the vessel were tinged black.

Other kinds, with cold water.

4. Agitation of these sorts of expectorated matter (3), in a large proportion of water at the temperature of 170° and upwards, produced a greater degree of milkiness, and a greater number of small masses, which could not be dissolved by long shaking. Putrefaction did not take place so soon in these mixtures, as in those in cold water.

And with hot water.

5. If the proportion of the last mentioned kinds of expectorated matter be two or three parts to one of cold water, or under the temperature of coagulation, a uniform mixture may be produced by violent agitation, the water being entangled by the viscidity of the matter, rather than chemically united.

Mechanical mixture.

6. On boiling the mixtures (5), a great part of the expectorated matter is separated in a curdy form from a milky liquid.

This boiled.

7. If less than two grains of expectorated matter were diffused through five hundred grains of water, no evident precipitation was occasioned by tannin; while with one grain of isinglass jelly, or white of egg, or of serum of blood dissolved in five hundred grains of water, there was an evident precipitation with this reagent.

Action of reagents.

8. I could arrive at no useful conclusions, for the distinction of expectorated matter from other coagulable, or

from

from any gelatinous substances by comparative trials with muriate of tin, nitro-muriate of gold, oximuriate of mercury, acetite of ceruse, and acetite of litharge.

SECT. V. *Agency of acetous Acid.*

Ropy opaque matter treated with distilled vinegar.

1. Twenty ounces of ropy opaque matter, by being shaken with ten pints of distilled vinegar, were so broken into a fibrous or even vascular form as to exhibit an organized appearance, the bulk being reduced to at least one third of the ropy matter. By repeated agitation and long digestion, the coagulated masses were broken into smaller pieces, but did not appear to be farther contracted in bulk, or to dissolve. With some parcels of matter the vinegar preserved its transparency, with others it became wheylike, the matters being deposited in a curdy state. The mucilagelike expectorated matter, or this mixed with the other kinds, afforded wheylike, or more or less turbid liquids with vinegar.

Liquid distilled

2. (*a*) The decanted liquid, and the liquid obtained by pressure of the sediments of the last mixture (1), being distilled to about one eighth, the remainder was evaporated to the consistence of a thick extract. The distilled liquid did not appear to have received any impregnation, except what had altered a little the odour. This extractlike residue amounted from one forty-fifth to one eightieth the weight of the expectorated matter, according to the kind of this substance. It varied also according to the proportion of the matter to the acid menstruum.

Extract.

Digested a 2d time,

(*b*). The residue (2. *a*) just mentioned, after a digestion a second time, in the same quantity of acid, afforded a smaller quantity of extractlike matter than before.

a 3d,

(*c*). The third digestion afforded still less of this substance.

4th, 5th,

(*d*) The fourth and fifth digestion gave somewhat less than the immediately preceding one.

and 6th,

(*e*). The sixth digestion yielded nearly the same proportion of extractlike matter as the fourth and fifth.

Insoluble matter incinerated.

3. The undissolved matter, after these repeated digestions in vinegar (1, 2), being exposed to fire in a platina crucible, first flamed and partially melted; they became apparently

apparently charcoal, which burned away to the state of a brown earthlike substance, scarcely $\frac{1}{100}$ of the weight of the substance subjected to fire, and not above $\frac{1}{1800}$ of the expectorated matter by which it was afforded. It consisted chiefly of phosphate of lime, with indications of carbonate of lime, of a sulphate, of a muriate, of silica, or at least vitrified matter, and of oxide of iron.

Extract partially deliquescent.

4. The extractlike matter, from the first digestion of the expectorated matter (2, *a*), by exposure to the air, in a few days partially deliquesced, affording no signs of alkalescency, but having a peculiar salt taste.

Treated with nitrous acid.

(*a*). A little of this deliquescing part being burnt to dryness, with a large proportion of nitrous acid, on beginning to be ignited, it deflagrated, leaving a blackish saline residue; which soon deliquesced, and being lixiviated, it precipitated supertartrate of potash with tartaric acid, and gave a reddish precipitate with nitro-muriate of platina. The residue also contained lime, for the dissolution in acetous acid afforded oxalate of lime, on the addition of oxalate of ammonia.

Digested in alcohol.

(*b*). This extractlike matter, (2, *a*), by digestion in rectified spirit of wine, gave a blackish tincture, which, being decanted and evaporated, left a residue. This became quite liquid after twenty-four hours exposure to the air. It consisted chiefly of acetite of potash, with an inappreciable portion of muriate of soda, and ammonia neutralized, probably, by phosphoric acid; beside uncoagulable and ungelatinizable oxide of animal substance.

Residuum.

(*c*). The undissolved matter by spirit of wine, just spoken of (*b*), after expression, being desiccated, it remained in a solid state after exposure to the air, only growing a little soft in four weeks time. By combustion, it afforded a difficultly fusible ash, which after fusion was found to consist chiefly of phosphate of lime, muriate of soda, with a little potash; a sulphate, traces of iron, and vitrified matter, which probably contained silica, united to the other substances manifested in this fused mass.

Extract from the 2d digestion.

5. The extractlike matter, by acetous acid on the second digestion, (2 *b*), grew soft, but did not deliquesce on exposure to the air. It was found to differ from the matter obtained

tained by the first digestion in the same menstruum, in containing a much smaller proportion of potash and muriate of soda, as well as of neutralized ammonia.

From the 3d. 6. The extractlike matter, from the third digestion in vinegar (2 c), differed from the former, in containing a still much less quantity of the salts just mentioned.

From the subsequent. 7. The fourth and subsequent digestions (2, d, e) afforded extractlike substances, which contained scarcely any thing but a very small proportion of earthy phosphates, and indissoluble vitrified matter, produced by incineration and fusion. It did not appear, that the oxide of animal matter, dissolved by the distilled vinegar in all the preceding digestions successively, was of different kinds; but it appeared, that its coagulable property was destroyed by dissolution in this menstruum. Accordingly, there is no reason to believe, that the whole of this oxide is not dissoluble in the acid here employed, although the requisite proportion may decrease after each digestion, within certain limits.

Opaque ropy matter treated with vinegar. 8. A few drops of opaque ropy matter being agitated in half a pint of vinegar, a number of fibrous masses appear, apparently one fourth or one fifth of the bulk of the matter added; and these fibrous forms subsist, notwithstanding continued agitation, totally disappearing only in consequence of long digestion in successive large quantities of this acid.

SECT. VI. *Some Experiments with different Objects.*

Potash neutralized by animal oxide. 1. To produce a synthetic proof, that potash may be neutralized by oxide of animal matter, I triturated ten grains of the exsiccated and coagulated part of expectorated matter, freed from all saline substance, with pure potash gradually added, and a little water. Several grains were in this way united, without any effect being produced by the compound on turmeric paper. More alkali was added till the compound barely manifested the existence of alkali to the test just mentioned. It was then digested in spirit of wine, to which it imparted a deep brown colour, and the tincture being distilled, it afforded a dry extract, which grew moist on exposure to the air, but scarcely affected turmeric paper.

On

On incineration, however, the alkali was denuded, and fusion was easily produced.

An equal portion of the animal oxide, of the same parcel as in the last experiment, was digested in spirit of wine, in the same circumstances as this oxide united to potash. It imparted no colour to the spirit, and the extract obtained was in smaller quantity, than in the preceding experiment. Being evaporated to dryness, the residue did not grow moist, but it became a little soft on exposure to the air. Being exposed to fire, it left an inconsiderable proportion of infusible residue, with barely traces of alkali and muriate.

Animal oxide digested in alcohol.

2. To determine, by a more satisfactory experiment than a preceding one, whether or not acid was united to the potash and evaporable, ten ounces of watery liquid, which separates from the curd on boiling expectorated matter, were evaporated to the consistence of a thin extract. This matter indicated neither acid nor alkali in a disengaged state, but it was ascertained to contain a large proportion of potash combined; and an acid smell was perceived on heating it with phosphoric or tartaric acid. Ten drops of liquid phosphoric acid were mixed with four hundred grains of this extractlike matter, and at a low temperature it was subjected to distillation to become almost a dry substance; but no acid could be detected in the liquid which came over, nor did this dry substance indicate any acidity to the usual reagents—neither on exposure to the air did it, as before the addition of acid, grow moist. Phosphoric acid was farther added, till it became sensible to the test of turnsole; but neither by elutriation nor distillation could any acid be obtained, except a small portion of the phosphoric acid by elutriation, the rest having united to the potash.

Examination for acid.

3. To furnish an estimate of the proportion of ammonia, I subjected to distillation a mixture of a pint of expectorated matter of the fifth kind, page 229, with three ounces of well burnt lime, but I could not reckon the ammonia in the distilled liquid at more than two cubic inches, or less than half a grain in weight.

Very little ammonia.

SECT.

SECT. VII. *Conclusions.*

Various kinds of expectorated matter differ only in the proposition of the ingredients.

1. From the preceding experiments and observations, and from others, which I might have related, it does not appear, that the various kinds of expectorated matter, page 217, differ in the ingredients of their composition, but merely in the proportion of them to one another.

These albumen, and water impregnated with salts.

2. It has been shown, that expectorated matter consists of coagulable, or, as it is also now frequently termed *albuminous* animal substance, and of water impregnated with several saline and earthy bodies—that the largest proportion of the animal substance, which may justly be called an oxide, amounts to one twelfth, and in some very rare cases to one tenth of the expectorated matter, reduced to a brittle state by evaporation; and that the smallest proportion of this oxide, in rare instances, amounts to one forty-fifth of the expectorated matter; but that the usual proportions of it vary between one twentieth and one sixteenth of this coagulable oxide to the evaporable water, that is, between five and six per cent of the expectorated matter.

The albumen in the state of oxide.

Impregnating substances.

3. The impregnating substances have been shown to be muriate of soda, varying commonly between one and a half and two and half per 1000, of the expectorated matter—potash varying between one half and three fourths of a part per 1000—phosphate of lime about half a part of 1000—ammonia, united probably to the phosphoric acid; phosphate, perhaps of magnesia; carbonate of lime; a sulphate; vitrifiable matter, or perhaps silica; and oxide of iron. But the whole of these last six substances scarcely amounting to one part in 1000 of the expectorated matter, it would be useless to estimate the proportion of each of them. It is very probable, that the proportions and quantities of these ingredients vary much more than now represented in different states of disease and health*. It is very probable also, that some of the ingredients may occasionally be absent, and others of a different kind be present, agreeably to the different states, on different occasions, of the other secretions.

*In one case, the opaque expectorated matter in a pulmonary consumption, having been exsiccated to brittleness, became almost liquid after a night's exposure to the air.

4. It

4. It is manifest, that the different states of consistence of expectorated matter are owing to the proportion of albuminous or coagulable oxide, but I purposely avoid giving an account of the different conditions of health, on which the difference of consistence depend.

Differences of consistence.

5. The thicker the matter, the smaller I commonly found the quantity of saline impregnation. Hence in sudden and copious secretions of the bronchial membrane, the matter is asserted to be salt, and to feel hot. In such instances the proportion of coagulable matter was small, but that of the saline impregnations, particularly of the muriate of soda, and neutralized potash, so great, that the exsiccated expectorated substance tasted very salt, and presently grew moist, or even partially deliquesced; but the opaque ropy or puriform matter afforded a much larger proportion of exsiccated residue, which was but slightly salt, and generally only became soft on exposure to the air. This property of growing moist depends on the potash.

The thickest contains the least salts.

6. Each of the human fluids, according to my experiments, contain neutralized potash; at least, this is the fact of the blood, dropsy fluid, pus of abscesses, and pus secreted without breach of surface; the fluid effused by vesicating with cantharides; the urine; and in course in the very abundant secretion from the nose by catarrh. The alkali being united to oxide of animal matter in these fluids, it is easily demonstrable.

Neutralized potash in all the human fluids.

7. Although I think I have discovered many properties, by which expectorated secretion may be distinguished from expectorated pus, I shall not speak of them, on this occasion, further than just to observe, that the saline impregnation of pus, particularly that of potash, and muriate of soda, is in very much less proportion than in expectorated secretion; and hence it does not become moist after exsiccation, on exposure to the air.

Expectorated secretion more saline than expectorated pus.

8. It has been, I believe, uniformly asserted, that the circulating and secreted fluids are impregnated with soda; that it is especially in the matter secreted by the bronchial membrane. The experiments of others must confirm or disprove mine. It seems, however, much more reasonable, that the human fluids should be found to contain potash than soda,

No uncombined soda in the fluids.

soda, united to some oxide or destructible acid; because the former alkali is daily introduced with the vegetable food, and with the drink of fermented liquor; and it is as little likely to be destroyed, as the muriate of soda also induced in the very same way. But our food and drink do not, commonly at least, contain the soda united to a destructible acid, or an oxide.

Expectorated matter not a mucous fluid.

9. It is plain, from the preceding experiments, that expectorated matter belongs to the class of coagulable fluids, and not of gelatinizable, or, as commonly asserted, mucous fluids. It differs from the coagulable fluid, serum of blood, in forming a much thicker fluid with a much larger proportion of water: for serum, and also the water of blisters, are quite liquid, although they afford on exsiccation from one twelfth to one eleventh of their weight of brittle residue; while some kinds of expectorated matter, of the consistence of mucilage, afford only one fortieth of dry residue; and others, of the consistence of thin paste, afford only one fourteenth of residue.

Full of globules, which seem to indicate organization.

10. But for the unavoidable extent of this paper, I should trouble the learned Society with various other conclusions and remarks, especially concerning the *globularity* of expectorated matter, which seems to indicate organization. Although Antonius Van Lewenhoeck, above a century ago, discovered the globularity of blood, and even noticed it in other animal fluids, neither he, nor any other person, as far as I know, investigated the subject in any fluid but the blood, till by Mr. Home's acuteness and industry, at a very early period of life, it was observed in pus. I have in this paper related, that expectorated matter, especially the opaque ropy kind, as well as the puriform, is full of globules, and that, except by such agents as destroy charcoal, they are scarcely destructible. Do these spherical particles consist chiefly of organized carbonaceous matter?

Are they organized carbonaceous matter?

V.

V.

Abstract of chemical Experiments on the soft Roe of Fishes; by Messrs. FOURCROY *and* VAUQUELIN*.

THE memoir of Messrs. F. and V. is divided into five sections. In the first the authors give an account of several experiments made with the roes of carp, in order to ascertain their principal properties: in the 2d they examine the phenomena, that take place when they are burned in an open fire; in the 3d, the products they yield in distillation, and the nature of the coal left in the retort; in the 4th, the manner in which they comport themselves with cold and boiling water; and with alcohol: and the 5th contains a recapitulation of the heads of the preceding experiments, with some applications of them to physiology.

Method pursued by the authors.

SECT. I. *Preliminary experiments.*

1. The roe is distinguished from the other organs of fishes by its soft consistence; by its texture, which is somewhat greasy and smooth to the touch; and particularly by its fishy smell.

General characters of soft roe.

2. It is neither acid nor alkaline.

Neither acid nor alkaline.

3. When triturated with a concentrated lixivium of potash it emits no smell of ammonia; and on the addition of a fresh quantity of alkali it forms a thick magma.

4. Thirty gram. [463 grs] of roe, dried in the air by a gentle heat, being mixed with 6 gram. of potash, and afterward diluted with water, afforded when distilled merely some traces of volatile alkali, which evidently arose from a small quantity of muriate of ammonia, that naturally exists in the roe.

These two experiments prove, that the roe contains no perceptible quantity of volatile alkali.

Contains no ammonia.

5. Roe dried slowly by a gentle heat in the open air loses three fourths of its weight. It turns a little yellow, and becomes friable.

Action of heat on it.

* Annales de Chimie, vol. LXIV, p. 5.

6. Heated in a platina crucible, it first hardens, then softens, and at length melts in great part. Yellow fumes are emitted, which have the acrid smell of animal fats.

The coal contains an acid,

7. The coal of the roe, being washed with warm water, communicates to it *a very decided acidity*. This solution gives a precipitate with alkalis: if it be evaporated to dryness, and the residuum treated with water, a white substance separates, consisting of the phosphates of lime and magnesia: if ammonia be added to it, ammoniaco-magnesian phosphate and phosphate of ammonia are obtained.

which is the phosphoric,

Water therefore extracts from the coal of the roe *free phosphoric acid,* and a little *phosphate of lime and of magnesia.*

The phosphoric acid obtained certainly did not exist in the roe, for this is not acid. On the other hand the roe contains no volatile alkali, that can be made evident to the senses. Now as the phosphate of ammonia is the only salt, that can yield phosphoric acid by the means of heat alone, we must conclude, *that this acid is formed during the combustion of the roe,* which is a very remarkable fact, and altogether new.

and formed during the combustion.

Roe calcined

SECT. II. *Combustion and calcination of the roe in an open fire.*

Perforated the platina crucible.

1. One hundred and seventy-eight gram. [2748 grs.] of fresh soft roe yielded 7·8 gram. [120 grs.] of coal. This coal, strongly calcined in a platina crucible, grew red, and perforated the crucible. The metal was rendered brittle.

Yielded phosphate of lime,

2. The aqueous lixivium of the coal was acid. On evaporation it let fall 45 centig. [7 grs.] of phosphate of lime. The lixiviated coal weighed only 5 gram. [77 grs.]

phosphoric acid,

3. The lixivium deprived of phosphate of lime, and saturated with volatile alkali, yielded 5 gram. [77 grs.] of dry phosphate of ammonia.

and phosphate of magnesia.

4. This phosphate of ammonia contained a little phosphate of magnesia; for, being heated before the blowpipe, it yielded a transparent pearl, which became opake on cooling, and was not wholly soluble in water.

Phosphorus formed from it.

5. On distilling 5·6 gram. [86·5 grs.] of this phosphate of ammonia with 1 gram. [15·4 grs.] of charred cork in a coated

coated glass retort, 26 cent. [4 grs.] of phosphorus were produced. The residuum of the distillation weighed 4·2 gram. [65 grs.], and still contained a great deal of phosphoric acid. The muriatic acid, with which it was washed, yielded, on the addition of limewater, 4·5 gram. [69·5 grs.] of phosphate. It contained a little phosphate of magnesia.

6. Messrs. F. and V. always remarked, that the coal of the roe calcined some time, and afterward once lixiviated with water, yielded an acid liquor containing a little lime, and a great deal of magnesia. These two phosphates are easily separated by evaporation to dryness, and applying water to the residuum, when the phosphate of magnesia will dissolve, and that of lime be left behind. Ammonia separates from this solution but a very small quantity, because it forms a soluble triple salt with phosphate of ammonia.

Separation of the phosphates of lime and magnesia.

SECT. III. *Distillation of the roe, and examination of its coal.*

1. The apparatus employed for this purpose consisted of a well tried stone retort, communicating, by means of an adopter, with a glass receiver, from the tubulure of which issued a curved tube terminating in a phial full of oximuriatic acid. This was for the purpose of knowing whether there were any phosphorus in the gasses evolved. The fire was managed with a great deal of caution, and raised gradually till the bottom of the retort was at a white heat.

Roe distilled.

123 gram. [1899 grs.] of fresh roe yielded the following products:

Products.

a, A great deal of colourless water;

b, Some white, or slightly lemoncoloured, oil;

c, An oil as red as blood, and tolerably fluid;

d, A blackish brown thick oil;

e, At the same time with this oil some salts, which condensed in needly crystals on the sides of the adopter;

f, A white crust, mottled with yellow and red, adhering to the upper part of the adopter;

g, There was but little carbonic acid and carburetted hidrogen gasses evolved.

2. We shall now examine the nature of these products.

Prussiate of ammonia.

The water (*a*) contained some carbonate, a great deal of prussiate, and some traces of muriate of ammonia.

Phosphorus.

The crust (*f*) was *pure phosphorus*, for it smoked on the contact of air, diffused the smell of phosphorus, was luminous in the dark, and inflamed rapidly on increasing its temperature.

The Oils examined.

The oils (*b, c*), heated with nitric acid at 30°, emitted white fumes having a smell of phosphorus mingled with that of oil. The capsule in which this operation had been performed, was luminous in the dark. In order to collect the phosphorus, the liquor was poured into a glass retort, and the distillation urged, till the greater part of the oil was destroyed by the action of the nitric acid. Some water passed over into the receiver, containing carbonic, prussic, and muriatic acid. A light oil likewise came over. These products contained no phosphorus. The oil that remained in the retort was partly converted into a red bitter matter, partly into a kind of wax, which congealed on the surface of the liquor in cooling.

The nitric solution, evaporated to the consistence of a sirup, afforded crystals of nitrate of ammonia. The mother water contained phosphoric acid, proceeding from the phosphorus acidified by the nitric acid.

The coal.

The coal left after the distillation of the roe weighed 7·5 gram. [116 grs.]. It was not acid; did not become so by calcination; and did not inflame during this operation.

Phosphorus an elementary part of roe.

This experiment shows, 1, that the coal of distilled roe is not phosphuretted, like that of roe calcined by a gentle fire in the open air: 2, that phosphorus is an *essential element of roe*; a fact that has never been mentioned by any one, nor has its existence as an element in any animal substance ever been suspected.

With less heat the coal a phosphuretted carburet, and scratches glass.

3. When the heat is not carried so far as in the preceding distillation, no phosphorus is obtained, it remaining combined with the coal in the state of a phosphuretted carburet. This coal is the hardest afforded by any organized substance, for it scratches glass. When heated gently in a platina crucible, a yellowish green flame is produced; and when the temperature is raised to a dull red, this flame becomes intermittent

termittent. After this calcination the phosphorus is acidified, and may be dissolved in water.

Desirous of removing any doubt, that might arise respecting the state of the phosphorus in the coal of roe, the authors lixiviated this coal with muriatic acid. This dissolved the phosphates of lime and magnesia; and the residuum, calcined afresh, exhibited the same phenomena as it did before it was thus treated. The phosphorus of the coal of roe, therefore, is not owing to the decomposition of these phosphates.

The phosphorus in the coal not owing to the decompo-sition of any phosphate.

The affinity of carbon for phosphorus appears to be very great; for the same coal, being calcined four times, yielded phosphoric acid each time.

Carbon has a strong affinity for phosphorus.

Beside the phosphates of lime and magnesia, which the coal of roe retains, we find also phosphate of soda and of potash, which may be dissolved in water. It contains nitrogen too; for when it is calcined with potash we obtain prussiate.

Phosphate of soda and of potash, and nitrogen.

4. The coal of fibrin, subjected to the same experiments as that of roe, does not exhibit the same phenomena. The water with which this coal was washed was perceptibly alkaline; which shows, that the acidifiable property observed in the coal of calcined roe does not belong to all animal coal; and there is even reason to believe, that it is peculiar to this substance.

Coal of fibrin.

SECT. IV. *Roe treated with water and with alcohol.*

1. Roe triturated with distilled water, and reduced to a pulp, imported to it no signs of acid or alkali. Its soft and white part is diffusible in water, and gives it the appearance of an emulsion; the membraneous matter however is perceptible in it, and cannot be separated; and when filtered, it is still turbid.

Roe treated with water.

2. If we boil the water in which roe has been diffused, there is one portion which coagulates like albumen; but the water retains in solution a matter analogous to gelatine, for it becomes a jelly by boiling down.

Contains albumen and gelatine.

3. This jelly, being burned and calcined, exhibited no phosphorescence,

Jelly.

phorescence, or phosphoric acid; only sulphuric acid extracted from its coal phosphate of lime and of magnesia.

Soluble portion.

4. The soluble part of roe is precipitable by nutgalls and several metallic solutions.

No phosphate of ammonia.

5. Roe being boiled a long time in water, not an atom of phosphate of ammonia was extracted; which confirms what has been said above of the existence of phosphorus in a pure state in this animal substance.

Insoluble part.

6. The portion of roe not soluble in hot water being carbonized, and then lixiviated with water, it yielded no phosphate of ammonia. It afterward comported itself as the coal of the entire roe. The phosphorus, therefore, remains with the albumen.

Roe treated with alcohol.

7. Alcohol takes up from roe a kind of saponaceous matter, which imparts to it an unpleasant smell and taste.

When this alcohol is distilled, the liquor becomes of a greenish yellow; and, when all the fluid has passed over, a substance remains resembling soap both in taste and smell. This soap retains some atoms of alkaline phosphate.

The roe treated with alcohol has no longer a greasy feel, but is dry and harsh. It appears therefore, that its unctuosity is owing to the animal soap.

SECT. V. *Conclusion.*

[illegible]y of [illegible]phospho[illegible] organi[illegible]stances.

1. The discovery of phosphorus in the combustible state in organized substances belongs entirely to Messrs. Fourcroy and Vauquelin*; for, if we consult those chemists who have attempted the analysis of compounds of this kind, we find only Margraff, who says he obtained phosphorus in distilling a vegetable substance. Mustard seed, with which he made his experiment, being subjected to a fresh examination, did not furnish the slightest trace of this combustible. Hence it is probable, that the phosphorus obtained by this chemist was an accidental product of the operation.

Mustard seed.

Proposed inquiry.

2. The authors of this paper purpose to inquire, whether this phosphuretted animal compound belong to the organization of fishes in general, or be peculiar to their soft roe.

Phosphores-

They presume, with much reason, that the phosphorus

* This is a mistake. See the following article. C.

found

found in an organ so essential has some influence in the phosphorescence of fish; and that hereafter perhaps we may discover this singular property to be owing to the same cause, not only in several marine animals, but also in several insects that dwell upon land.

cence of fish and other animals.

VI.

On the Phosphorus, that Seeds afford by Distillation; and on the Decomposition of Alkaline Phosphates by Charcoal: by THEODORE DE SAUSSURE *.

Phosphorus in animal

SECT. 1. THE interesting inquiries of Messrs. Fourcroy and Vanquelin on the roe of fishes published in the Annales de Chimie †, and their discovery of phosphorus in the combustible state among the products of the distillation of this animal substance, led to the observations that form the subject of this paper.

and vegetable substances.

In the account of their labours it is said, that phosphorus had never before been supposed to be found in any organized substance, except by Margraff in mustard seed; and this they conceive to have been accidental, as their examination of this seed gave no trace of it ‡.

Margraff not the only chemist that found phosphorus in seeds.

The silence of modern writers on this discovery of Margraff seems to render it questionable; since the observation appears too singular and striking, for no one to have attempted to confirm it and investigate its cause. That chemist however does not claim for himself the discovery of phosphorus in the distillation of seeds. He first speaks of Albinus and Hoffmann, who had before obtained phosphorus by distilling the seeds of mustard, rue, and rocket, with a very vehement fire. He afterward mentions Pott, who had taught him, that wheat, rye, and other grain, yielded phosphorus. And lastly he relates his own results, which confirm those of his predecessors. Such testimony could have left no doubt of the reality of the observation;

* Annales de Chimie, vol LXV, p. 189.

† Vol. LXIV, p. 5. See the preceding article. ‡ See p. 278.

if the considerations I have adduced above had not led modern chemists to adopt a contrary opinion.

Beans appear to contain phosphorus.

Several years ago, as I was incinerating some beans in a platina capsule, I exposed them suddenly to too strong a heat. The charcoal and ashes of these seeds agglutinated together at the bottom of the capsule, and a hole was made through the metal in one place where it was in contact with the charcoal. I did not then ascribe the effect to its true cause; but when I found Messrs. Fourcroy and Vauquelin had experienced a similar incident when treating fishes roes in a platina vessel, I concluded, that the observations of Pott, Margraff, &c., were well founded. As the success of the process depends in great measure on its minutiæ, I shall relate all the particulars of that I adopted.

Wheat distilled.

I subjected to distillation in a stone retort 1·039 kil. [16042 grs.] of wheat. The fire was at first very gentle, and raised by degrees, in the course of three hours, to a slight white heat. When at this temperature no more vapour was evolved, I took out the coal that had formed in the retort, and which weighed 250 gram. [3860 grs.] This I powdered and introduced into a small coated porcelain retort, which it three parts filled, and which communicated by means of an adopter with a tubulated receiver half full of water. After having luted all the junctures of the apparatus, I placed the retort in a furnace, the fireplace of which, 2·3 dec. [9 inches] broad, terminated in a tunnel 1·3 dec. [4½ inches] in diameter, and 2 met. [6 feet 6 in.] high; and I raised the fire gradually in about two hours to as intense a heat as the furnace would produce. This heat was sufficient to fuse the argillaceous lute that coated the retort, and remove almost the whole of it. A white smoke, having the smell of phosphorus, then diffused itself through the receiver. When the apparatus was cold, I unluted it, and found a slight coating of phosphorus in the adopter. It took fire on the admission of air, and exhibited the smell, colour, consistency, and all the other characters, by which this eminently combustible substance is so easily distinguished.

Phosphorus in the adopter.

The success depends on the management of the heat.

From this result we may presume, that other seeds would furnish the same product; and that, if it have not been obtained by all chemists, it was because they did not employ

ploy a degree of heat high enough, a sufficient quantity of seed, and all the precautions I have mentioned above. I have reason to think for instance, that, if the degree of heat requisite to disengage the phosphorus be employed at the commencement of the operation, this product will not be obtained separately, because it would combine with the hidrogen gas of the coal.

The coal of wheat does not contain free phosphoric acid or phosphate of ammonia.

The coal of wheat, treated with boiling water before the extraction of the phosphorus, yields a lixivium that turns sirup of violets green, and is indebted for this property to potash. In this instance therefore the phosphorus appears not to be owing to the action of the charcoal on free phosphoric acid. Neither does it arise from the decomposition of phosphate of ammonia; for the heat requisite to carbonize the seed appears to be more than sufficient to volatilize this salt.

Considerable quantity of phosphate of potash obtained from all seeds.

As I found a considerable quantity of phosphate of potash in all the ashes of the seeds I have analysed, particularly in wheat (see my Chemical Inquiries concerning Vegetation), I thought it necessary to examine, whether this salt be decomposable by charcoal. I confess however it is not certain, that this phosphate exists already formed in the coal made in close vessels; for the water with which it was lixiviated afforded me none, or at most scarcely perceptible traces of this salt; but the coal, from its great porousness, retains substances intermingled with it so pertinaciously, that we can conclude nothing from this operation.

SECT. 2. *Decomposition of the Phosphate of Potash by Charcoal.*

Phosphates decomposable by charcoal.

Those of potash, soda, and lime, supposed not.

Among the neutral phosphates we know only the phosphate of ammonia, and those of some metals, that are decomposable by charcoal. All the chemists who have engaged in these inquiries have asserted, that phosphate of potash, phosphate of soda, and phosphate of lime without excess of acid, resisted this decomposition. It is on a supposition therefore, that these authors might have been mistaken, that I made the following experiment.

Phosphate of potash decomposed by charcoal.

I neutralized pure potash with phosphoric acid prepared by the action of nitric acid on phosphorus. To prevent all suspicion

suspicion of excess of phosphoric acid, I added a slight excess of alkali to the phosphate of potash, so that its solution turned blue colours green. This phosphate, dried at a red heat, weighed 30 gram. [463 grs.]. It was powdered and triturated with twice its weight of beech charcoal, which had been red hot just before they were mixed. I put the whole into a small coated porcelain retort, and exposed it for four hours, in the furnace described in the preceding section, to the same degree of heat as in the preceding operation. The phosphorus flowed by drops into the water of the receiver; and, though I extinguished the fire while some phosphoric fumes were still evolved, I collected in the receiver and in the adopter, 2·5 gram. [38 grs.] of phosphorus. In this weight I do not include a portion of this substance, which adhered to the neck of the retort; what was suspended in the water of the receiver, and rendered it yellow like whey; or what was combined in the phosphuretted hidrogen gas produced in this distillation. If it be considered, that 30 gram. of phosphate of potash contain by calculation about 4·8 gram. [74 grs.] of phosphorus only, there can be no doubt, that this salt was almost wholly or in great part decomposed by the charcoal.

SECT. 3. *Decomposition of Phosphate of Soda by Charcoal.*

Phosphate of soda decomposed by charcoal.

For this experiment I employed crystallized phosphate of soda, with a slight excess of the alkali, according to the common practice of the shops for obtaining regular crystals. By drying at a red heat 100 parts of this salt were reduced to 40·94. I powdered 30 gram. of this phosphate, and mixed with it 60 gram. of beech charcoal, which I made red hot immediately before mixing them. The operation was conducted as in the preceding instance, and I collected the same product, namely 2·5 gram. of phosphorus, with the same omissions as before.

As the phosphates of potash and of soda deprived of their waters of crystallization contain nearly similar quantities of phosphoric acid, we may deduce from them the same conclusions.

Why other chemists have failed.

The decomposition of the phosphate of soda by charcoal has been a particular object of inquiry to chemists. It is to be

be presumed therefore, that they failed either because they did not deprive this salt of its water of crystallization, or because they did not employ a degree of heat much beyond what is necessary for obtaining phosphorus from free phosphoric acid.

SECT. 4. *Decomposition of Phosphate of Lime by Charcoal.*

Phosphate of lime decomposed by charcoal.

I mixed an aqueous solution of nitrate of lime with phosphoric acid, and precipitated the solution with ammonia, which separated from it the phosphate of lime. Of this salt, well washed, powdered, and dried at a red heat, 10 gram. [154 grs.] were mixed with twice their weight of beech charcoal powder, which had previously been boiled in a large quantity of water to deprive it of potash.

The degree of heat employed in the preceding operations was not sufficient to render the result of this very decisive. I therefore gave up all thoughts of extracting the phosphorus by distillation in the furnace above described; and put a similar mixture of charcoal and phosphate of lime into a Hessian crucible, closed with a platina cover, and surrounded with charcoal powder in another crucible closed with an earthen cover. This apparatus was exposed for two hours to the action of a wind furnace, which produces the highest degree of heat, that Wedgwood's pyrometer will indicate. The interior crucible was not altered, but its platina cover was fused. The earthen cover remained entire. The residuum of the mixture of phosphate with charcoal was digested in muriatic acid. The solution, filtered and precipitated successively with ammonia and carbonate of ammonia, yielded to the former 4·52 gram. [70 grs.] of phosphate of lime dried at a red heat, and to the second 5·22 gram. [80 grs.] of carbonate of lime. The result shows, that about half the phosphate of lime was decomposed by the charcoal.

Native phosphate more difficult to decompose.

I repeated the preceding experiment on native phosphate of lime, the transparent Spanish chrysolite in regular crystals *. As the elements of this substance are much more

* Mr. Vauquelin has found, that 100 parts of chrysolite contain 53·3 of lime, and 45·7 of phosphoric acid.

condensed

condensed than those of the artificial phosphate, its decomposition could not be expected to proceed so far. From 6·26 gram. of chrysolite, powdered and mixed with twice its weight of powdered charcoal, I obtained, by the same process as above, 1·22 gram. of carbonate of lime, and 4·95 of phosphate of lime.

Beech charcoal.

I should mention, that 20 gram. of beech charcoal powder, treated with muriatic acid, yielded only 5 thousandths of a gramme of phosphate of lime, and 6 thousandths of a gramme of carbonate.

All organized substances will yield phosphorus.

From these inquiries it appears, that there is no animal or plant, but may yield phosphorus, or at least phosphuretted hidrogen gas, by simple distillation with a very vehement fire; for we know none of those substances, that do not furnish phosphate of lime and charcoal.

Degree of heat important.

It follows too, that the quantity of the phosphates obtained by incineration may vary, according to the degree of heat employed in the operation.

VII.

A simple and secure Method of joining long or bent Metallic Pipes, for Chemical Purposes; with some Queries. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Dr. Henry's juncture for metallic pipes

IT is but lately, that I have made chemistry a part of my studies, and it is therefore with considerable diffidence, that I take the liberty of addressing you. In reading Dr. Henry's account of a portable copper boiler, for exhibiting the most important facts respecting latent caloric*, I could not help thinking, that a more simple, and at the same time more secure, method of joining together long or bent metallic pipes, might be adopted, than by means of the stuffing box described in the passage alluded to. The plan which I propose to adopt consists merely in having a small

Different one proposed.

* See Epitome, p. 487, 5th edition.

tube

tube made, of the same materials as the larger ones, with a screw at each end, which is to be made to screw either into or over them. These screws must be cut in different directions, so that by turning the small tube in one direction, they will at the same time screw into both the long pipes. Should one upon trial be found to screw up to the shoulder sooner than the other, it will be requisite to file a small piece off the end of the longer top, and by this means they may be made to fit accurately, and to screw up exactly at the same instant. If the screw be cut upon the lathe, it will only be necessary to move the tool from left to right, instead of from right to left, as in cutting a common screw.

At present I have only tried this in a very rough manner, but from what I have seen, I have not the least doubt but it will perfectly answer the end proposed.

Query on oxigen containing carbon, and the evolution of hidrogen from potash & iron filings.

Permit me now to ask an explanation of two passages in your excellent Journal, from which I have derived much pleasure and information, and for which I am happy to embrace this opportunity of thanking you. The one occurs in page 56 of vol. XXIII, No. 101, May 1809, and is as follows: "Oxigen is found to contain carbon." The other passage is met with in page 150 of vol. XXII, No. 97, for the same year. They are the words of Mr. Davy—"When "melted potash is slowly brought into contact with turn- "ings or iron filings heated to whiteness, hidrogen gas is "evolved." Now as I cannot suppose, that water can exist in any sensible quantity in either melted potash or iron filings heated to whiteness, I am at a loss to account for the presence of hidrogen; especially when I recollect, that potash acted upon by the galvanic battery evolves oxigen. With respect to the first assertion, that "oxigen contains "carbon," I have not met with it before in any of the treatises on chemistry which I have read; but if either yourself or any of your numerous correspondents would have the goodness to explain these circumstances to a very young chemist, or to inform him where he may meet with a satisfactory explanation of them, he will esteem it a very great favour, and feel himself much obliged by their kindness.

Electrical experiment.

I have only to add one thing more. In page 312, No. 99,

Electrical experiment.

99, in a paper on electrical attractions and repulsions, the author, in mentioning the experiment with the tumbler and balls, says, "the glass held between the two hands is nothing more than a Leyden phial." I must beg leave to add, that any one may convince himself of the truth of this assertion, by only using a glass jar capable of holding a pint and half, or two pints, instead of a tumbler. If, after grasping it in both his hands, and inverting it over the pointed brass rod inserted into the conductor, he removes one hand, and puts it quickly to the bottom of the jar, he will receive a shock. The first time I tried this experiment, I received so strong a shock, that I was afraid of repeating it for some time. However, I have repeated it several times this afternoon, and in the presence of a second person, who also received several shocks from it: once in particular he complained of having a strong shock through the stomach, and at the same moment I had observed a flash pass between his stomach and the middle of the glass jar. I have frequently received several small shocks, but if it discharges itself at once, the shock is so strong as to be exceedingly disagreeable. A larger jar no doubt would produce more powerful effects. If any thing I have said appears to be worthy a place in your Journal, I shall be happy to see it inserted; at all events an answer to the queries proposed will much oblige, Sir,

Your obedient humble servant,

L. O. C.

ANSWER.

Junctures for metallic tubes.

The juncture recommended in the preceding letter will no doubt answer on many occasions, but my correspondent will not find it a very easy matter to make it fit with accuracy. If the middle part of the connecting tube were octagonal, it would admit of the use of a key, which should be made with a joint in the middle, to screw or unscrew it; and this would often be of advantage. The rim of Dr. Henry's stuffing box, which is certainly a preferable juncture, might likewise be made of this shape for the same purpose.

I know

I know no authority for the assertion, that oxigen contains carbon. It was no doubt founded on some mistake.

With regard to the appearance of hidrogen in Mr. Davy's experiment, I can only refer my correspondent to what has been said by that gentleman in his different papers on the decomposition of the alkalis, particularly in vol. XX, p. 323; XXII, p. 61 & foll.; XXIV, p. 105.

VIII.

Methods of raising the Bodies of Persons who have sunk under Water, or of assisting Persons in Danger in Water. By John Miller, *Esq., of Bedford**.

SIR,

WITNESSING, as many others did, a boy losing his life in the river in this place, for want of an expeditious means of finding and recovering his body when sunk, I wrote to and waited on Dr. Hawes, requesting him to procure for me whatever were the proper and most approved machines for that purpose. He purchased for me two sets of the Royal Humane Society's Apparatus for Reanimation; two of Daniel's Life Preservers; one of the Society's Rope Drags; and one of Dr. Cogan's Pole Drags; pointing out this last as the most approved and most efficacious machine then in use: and he gave me a sketch of a bar drag, armed with tenter hooks, and weighted with a bar of iron, to which the rope for drawing it was to be fastened.

Boy drowned.

Apparatus for recovering drowned bodies.

As soon as I had received it, I went with a medical gentleman, who had been eye witness to the fatal catastrophe, and who assisted in searching for and taking out the boy, (consequently knew the precise spot wherein he fell, and where he was found, which was within three feet of where he fell) to try the efficacy of Dr. Cogan's drag. The boy fell from off the bridge while fishing, and so far distant

Inconvenience attending Dr. Cogan's drag.

* Trans of the Society of Arts, vol. XXVII, p. 125. The gold medal was voted to Mr. Miller for these inventions.

from

from the shore as to render a boat necessary for us to reach the spot. The depth of the water was ten feet, and we found a difficulty, from the buoyancy of the pole in Dr. Cogan's drag, in forcing it to the bottom ; and when we did, the stones, or whatever it met with, so favoured its buoyancy, as to toss it up considerably when drawing forwards, and it required the force of both hands to keep it down so as to scrape the bottom with it. The result was, that the force the person holding the drag was obliged to use to counteract the buoyancy of its pole (added to the weight and resistance of the water against it) overpowered the means of the person who had the guidance of the boat, to move it in the direction he wished ; and instead of the movement of the boat directing the drag (or rather the person holding it) the drag, by the force required to keep it at the bottom, and by the weight and resistance of the water acting on its pole, governed the boat, and was as it were an anchor to it, confining the efforts of the person standing in it and using the drag, and making the action of the drag stationary instead of progressive.

Inconvenience of the bar drag.

The extreme scope of this drag not exceeding eighteen inches, may it not require too much time to traverse the uncertain space wherein the object to be searched for lies, to insure its success? The bar drag, of which Dr. Hawes gave me the sketch, in point of extent and its consequent expedition might answer where the bottom is level, and no ridges or hollows would occur; but, as being a straight piece of wood, it can accommodate itself to no unevennesses, consequently must leave holes, the most likely repository of the object sought for, unsearched.

Spot where a body lies uncertain.

One cannot but be aware, that it will seldom happen, that the precise spot can be ascertained. Either from the current of the water, the confusion or difference of opinion among spectators, the space to be searched is generally considerable and uncertain. Adverting, therefore, to what Dr. Hawes had sent me, and to the conversation I had with him, as well as the inquiries I had made, I thought something was still wanting; and the desideratum appeared to me to be a machine that would be expeditious, because it was extensive; and secure, because by accommodating itself to the ground,

Desideratum.

ground, however uneven, it would search holes and hillocks equally. I turned my mind, therefore, to form a machine of this description, and thinking that the buoyancy of the wood would take off from the weight of lead and iron about it, were I to extend it to ten feet in length, I determined on that length for it, and I found my expectation realised in the result. I therefore denominated it a machine or drag, easily drawn by one person, that fishes an extent of ten feet at one sweep, with the certainty of finding a body, if it lies within that space, let the ground be ever so uneven, or the water ever so deep.

Machine answering it.

Permit me, Sir, to request you to submit this account of my machine to the Society of Arts &c., accompanied with models and a drawing; and should they be honoured with the approbation of the Society, I shall feel myself much gratified, as their sanction could not fail to promote their publicity.

I have the honour to be,

Sir,

Your most obedient servant,

JOHN MILLER.

Description of Mr. Miller's Apparatus for Raising the Bodies of Persons sunk under Water. Pl. VI. Fig. 1, 2, 3, 4.

This machine consists of a round piece of deal A A, fig. 1, ten feet in length, and two inches and a half in diameter; at thirteen inches from each end of it, a square piece of deal B, twelve inches in length, and one inch and a half in diameter, made firm by a bracket, is let in and glued or nailed. To this bar four six-pointed drags, C C C C, are suspended at equal distances. These drags are weighted with two pounds of lead affixed or run on to the lower end of their shafts or stems, to steady them when in action, and to keep their points from running into the ground, which had they nothing to counteract their weight and preponderancy at top they would do. The buoyancy of the bar on the one hand, and the weight of the lead at the bottom of the drags on the other, has the effect of keeping the drags

Apparatus for raising drowned bodies described.

Apparatus for raising drowned bodies described.

in an upright position when at rest in the water, and in a diagonal one when pulled forward, scraping the ground, but not entering it. Each drag has a swivel at both ends of its shaft or stem. Its whole length, including swivels, is about nineteen inches. At nine inches and a half from the top, the hooks, which are three only at their base, but which are subdivided at eight inches from their ends, take their rise. They are curved, and their points when turned up again are about four inches below the level of their tops, and thirteen inches asunder; and the outside point of each sub-division is thirteen inches from its adjoining one. The extreme points are split and formed into a double hook, very sharp and pointing towards the stem.

Holes are bored through the bar A at equal distances, so as the hooks when suspended may approach each other within five inches. Through those at the end, which are larger than the others, and made close to the pieces of wood let into the bar, the principal or drawing ropes D D pass. This rope is of considerable length and strength, and goes through the top swivels of all the drags. It is then made fast by wooden wedges driven into the holes through which it passes, at such a length as will suspend the two end drags a few inches below the end of the pieces of wood let into the bar. The other two drags are suspended at the same distance from the bar by lines of an equal length coming through the holes in the bar, and tied to their top swivels. These two drags, as well as the two end ones, are made fast to the principal or drawing rope at equal distances with a piece of tar-line tied to their top swivels. And the two outside drags are kept in their proper situation by the principal rope going through a staple fixed in the pieces of wood let into the bar; and the two others are kept either from approaching or entangling with one another, or the outside ones, by bored pieces of wood, *a a*, of equal lengths, placed between each drag at the bottom, through which and their bottom swivels a rope made fast to the bottom swivels of the two outside drags passes. The drags, however tied or fastened their swivels may be, always have their own rotary motion free, consequently their points by their own gravity will always assume and retain their proper position when

when in action. The bar clears the way for the drags, breaking and removing weeds or what else might otherwise impede their progress and action. The drags, being suspended to the bar and separated from each other by nothing but what will give way, are undulatory in their progress as the bottom is, but will yet preserve the full extent of their sweep.

Apparatus for raising drowned bodies described.

Thus formed, the machine is ready for use, and may be drawn in this shape backwards and forwards at pleasure; but should the water wherein it is to be used be thought to contain roots of trees, or any thing likely to occasion the necessity of drawing up or releasing any one of the drags from the obstacle it has met with, then another appendage is advisable: a bar E, less in substance than the leading one, but of the same length, and which, for distinction sake I call the floating bar. Holes are made through this bar at the same distance from each other as those in the leading bar, and ropes of equal length (either ten feet, or any other length that may be chosen) after having been tied to the bottom swivels of all the drags are to be brought through these holes, and there stopped, either by a knot or pieces of cork at their ends. By this means any particular drag may be got at, without altering the position of the others, for, as far as the flexibility of the rope in the intermediate spaces between the several drags will admit, each is free and independent of the other; and since, by means of these ropes, a parallelism is preserved from the leading bar to the floating one, the floating one of course brings into view the direction the one which is sunk is taking.

Should the current of water be strong, it would carry the floating-bar before the leading one in drawing down the stream. A rope *b*, therefore, weighted with a stone or piece of lead at its end, is requisite. This will act as a kind of anchor to it, will steady it, and keep it where it ought to be, behind the leading one. If bored pieces of deal *d d*, 15 inches long, are, after passing the ropes of the floating-bar through them, made fast by wooden wedges to those ropes, at three inches distance from the bottoms of the drags, they will by their buoyancy and tension prevent these ropes of the floating bar entangling round the points of the drags.

 With

Apparatus for raising drowned bodies described.

With the floating bar attached to it, the progress of the bar cannot be instantly changed from straight forward to retrograde. For without making a sweep something circuitous, the ropes would entangle. But if a rope is fastened either to the middle of the floating-bar, or to that rope which operates as an anchor to it, by means of this the whole machine may be drawn back, and the same sweep repeated as often as required.

Should the floating-bar, for the purpose of disengaging any particular drag, be thought unnecessary, but that it is desirable to know what direction the one sunk is taking, and that the drawing backwards and forwards is an object, the floating bar, provided the holes at the ends of it are made sufficiently large, will do this by changing its direction from being a following bar into that of a leading one by this means. Detach it from the drags by untying the ropes that connect it with the bottom swivels. Pull them out of the floating-bar, and then pass the two ends of the principal or drawing rope through the holes next the end of it, and let it slip down to the leading bar; its own buoyancy will bring it to the surface of the water, and the operation of a man's pulling the drawing rope, will, by compression, keep it there.

The cost of this drag and appendages is three guineas.

Fig. 2, represents the drag in the state in which it should be preserved ready for use, or the manner in which it should be carried from place to place, to prevent any part from entangling. G G is a pole over which that part of the apparatus marked A and B is laid, and on which the cords D D folded up are hung, the drags C C C C remain suspended on the nearer side of the pole, and the floating bar E is laid within the drags with the hollow tubes *d d* below it.

Apparatus for assisting persons in danger

So closely connected with this subject of life preservation are two other machines, which I have had made, that on the presumption that a description of them will not be unacceptable, I shall annex it.

One of them I call the reel safeguard, devised by me for the security of persons going to the assistance of a drowning person, or diving for them.

The

The other a missile rope, capable of being flung to a person in distress, at a considerable distance from the shore.

Rope to be flung to a person at a distance from the shore.

This missile rope, (fig. 4, plate VI,) 35 yards in length, is rendered buoyant by pieces of cork fastened to it, at intervals of three or four feet. It is made fast at one end to a wooden reel A, six inches in diameter, and sixteen in length, on which it should always be kept wound, to prevent the ropes kinking, and for being in readiness. But when used, it must be unwound, because the corks are an obstacle to its running off the reel in the throw, and it should be spread on the ground, or held in the hand, free from entanglement so as not to catch or impede the throw. The throwing end of the rope is fastened to a piece of wood B shaped like the but end of an oar, as no shape can be better devised for the purpose of throwing it from the hand. The person throwing it holds fast the reel in his other hand.

The cost of this is, seven shillings and sixpence.

Apparatus for insuring the safety of a person going to assist another drowning.

The reel safeguard consists of a rope or line thirty-five yards long, made fast at one end to a reel (A, fig. 3) six inches in diameter, and ten in length. To the other end of this rope, a brass or iron tinned ring, large enough to admit of the leather and buckle part of the shoulder-straps passing through it, is fastened. The other part consists of two straps B B, of strong sadler's web, 2 inches wide, crossing each other, and well sown together just above the pit of the stomach, and after leaving a sufficient space for admitting the arms, the ends on each side are fastened together. To the ends behind the left shoulder, a brass or iron tinned ring of an inch and half in diameter is well fastened, and to the ends behind the right shoulder, a buckle and strap 18 inches long is fastened. If the space for the arms to pass through is proper, and the ring behind the left shoulder, and strap behind the right, properly placed, the pull from behind will be so equal, that the brace will neither press on the pit or stomach, or the windpipe; two essentials in swimming and diving. To insure this safeguard being put on properly, when hurry requires the use of it, "*Over the left arm,*" "*Over the right arm,*" should be written or stamped on the inside of the web part with printer's ink. The person using this slips his arms into the brace, while another

another, having first passed the buckle and strap (which is at the end of the brace behind the right shoulder) through the ring at the end of the rope, puts the strap through the ring behind the left shoulder, and buckles it to the size of the wearer. This person keeps fast hold of the reel, while the wearer plunges into the water; and the facility with which the rope runs off the reel prevents its being any impediment either in swimming or diving.

When it is required to pull the person wearing it to the shore, it is not to be done by winding the rope on the reel, but by shortening the rope by passing one hand over the other as quick as possible. This will pull the wearer on his back, and from the elevated position of the person pulling, whether he is on shore or in a boat, the wearer's head and shoulders will be pulled out of the water, and the immersion of his head either from debility, his efforts, or any weight he may have hold of prevented.

The cost of this is eight shillings and sixpence.

IX.

On Respiration. By William Allen, *Esq. F. R. S. and* William Hasledine Pepys, *Esq. F. R. S.**

Nitrogen evolved when oxigen gas respired.

ONE of the most prominent features in our last communication† was the evolution of a considerable quantity of azote, when oxigen gas nearly pure was respired; and although a considerable part of this azote must undoubtedly be attributed to the residual gas in the lungs, after the most forcible attempt at expiration, yet the fact seemed to demand still farther investigation, it appearing of consequence to ascertain, whether the increase of azote was uniform throughout the latter stages of the experiment, or *solely* confined to the earlier periods.

Large propor-

By adverting to our former paper, it will be found, that in

* Philos. Trans. for 1809, p. 404.

† See Journal, vol. XXII, p 180.

in an experiment, where more than 3000 cubic inches of oxigen passed through the lungs in seven minutes and a quarter, 62 cubic inches of azote were found in the first 250 cubic inches expired, though the gas originally contained but 2·5 per cent, or only 6 cubic inches in this quantity; in the two next portions expired, consisting of 562 cubic inches, we found 56 cubic inches of azote, though this quantity of gas, before it was respired, contained only 14; these first portions were given off in about two minutes, and contained nearly 100 cubic inches of azote more than could be accounted for in the oxigen employed; hence it is plain, that a large proportion of the increase is evolved in the first periods of the process.

tion of nitrogen evolved at the beginning of the process.

Our intention was particularly directed to this point in the following experiment. The oxigen, procured as usual from hyperoxigenised muriate of potash, was found to contain four per cent of azote; the experiment was conducted in the same manner as the preceding ones, except that the tubes of the gasometers were filled with oxigen, and the gas was not merely passed *once* through the lungs, but breathed backwards and forwards. In order to prolong the duration of the experiment, which began and ended with a forcible expiration, portions of respired gas were preserved for examination from each of the gasometers, in the following order:

Experiment. Oxigen respired repeatedly.

No. 1.	244	No. 5.	230	No. 9.	252
2.	294	6.	266	10.	168
3.	282	7.	254		—
4.	266	8.	288		2544

The portion of oxigen remaining in the water gasometer of the original quantity, not employed in the experiment, was found upon trial to contain four per cent of azote, as before:

Summary of the Experiment.

Summary.

Bar.	Therm.	Cub. Inches of Oxigen inspired.	Cub. Inches of Gas expired.	Deficiency.	Time.
29·9	51	2668	2544	124	13 minutes;

here the deficiency was greater than we had ever remarked before; but on passing an equal quantity of common air from

Deficiency very great,

the apparatus perfect.

from the water gasometer, and registering it in the mercurial ones, we were satisfied that the apparatus was quite perfect. It is, however, to be considered, that the respiration in this case was not natural, and that some small degree of force was required when the inspirations and the expirations were made in the mercurial gasometers, which renders this experiment rather different from those which had preceded it: and it appears to us probable, that a portion of air was forced into the extremities of the bronchiæ, which could not be suddenly expelled by the strongest attempts at expiration. Hence also, perhaps, the constant though smaller deficiency, even when the air was only once passed through the lungs; but when the process is continued for a much longer time, it is probable that the vessels recover their tone, and are able nearly to expel the whole of the volume admitted.

The air expired in the present instance, being examined in the manner described in our last paper, we found that 100 parts from each of the gasometers contained the following proportions:

State of the air after respiration.

No.			No.		
No. 1.	10	carbonic acid	No. 4,	10	carbonic acid
	21	azote		7·75	azote
	69	ozigen		82·25	oxigen
	100			100	
No. 2.	10	carbonic acid	No. 5.	10	carbonic acid
	11	azote		7	azote
	79	oxigen		83	oxigen
	100			100	
No. 3.	10	carbonic acid	No. 6 to 10 mixed	10·5	carbonic acid
	8·5	azote		5·5	azote
	81·5	oxigen		84	oxigen
	100			100	

We shall first calculate the total quantity of azote existing in the gas before the experiment, and afterwards estimate what was produced in the different periods during the first half of the experiment.

Calcu-

Calculation for Azote.

2668 cubic inches of oxigen were employed containing four per cent azote:

Nitrogen calculated.

100 : 4 : : 2668 : 106·72

the total quantity of azote in the gas consumed was 106·72 cubic inches.

Azote found after the Experiments.

	Cubic Inches			Azote found
No. 1.	244	100 : 21	: :	244 : 51·24
2.	294	100 : 11	: :	294 : 32·34
3.	282	100 : 8·5	: :	282 : 23·97
4.	266	100 : 7·75	: :	266 : 20·61
5.	230	100 : 7	: :	230 : 16·10
6 to 10.	1228	100 : 5·5	: :	1228 : 67·54
			Total	211·80 cubic inches

The whole azote, found after the experiment, was .... 211·80 cubic inches
Azote detected by the same tests before the experiment only .... 106·72 cubic inches

Increase of azote 105·08

Increase of nitrogen.

Now, as the whole time was thirteen minutes, if we divide this by the number of gasometers filled, it will give us one minute eighteen seconds for each, and the following will be the periods in which the azote was evolved.

	Time. ′ ″	Azote found.		Azote in the Oxigen.		Increase.
No. 1.	1 18	51·24	less	9·76	equal to	41·48
2.	1 18	32·34	—	11·76	=	20·58
3.	1 18	23·97	—	11·28	=	12·69
4.	1 18	20·61	—	10·64	=	9·97
5.	1 18	16·10	—	9·20	=	6·90
6 to 10.	6 30	67.54	—	49·12	=	18·42
	13 min.	211·80		101·76		110·04

Here

Could this have existed in the lungs before the experiment?

Here the increase of azote appears rather greater, viz. 100 cubic inches, but the calculation in this case is made upon the gas *expired*, and from the above statement, we may see, that the evolution of azote goes on diminishing; we have sometimes even found, that towards the close of an experiment it has almost been reduced to nothing. The question now is, whether this increase of azote can be owing to the residual gas contained in the lungs at the beginning of the experiment, or whether a portion of oxigen is not actually exchanged for azote, when pure oxigen gas is respired.

or has it been exchanged for oxigen?

Here it may be useful to compare the azote found in our former experiments on oxigen, with the present.

Results of former experiments.

	Bar.	Therm. °	Oxigen Gas inspired.	Gas expired.	Deficiency.	Time. ′ ″	Quantity respired in a Minute.	Azote evolved.	Inferred Capacity of lungs.
No. 1.		53	3260	3193	67	9 20	348	110	141
2.	30·3	70	3420	3362	58	7 25	461	177	225
3.	30·15	70	3130	3060	70	8 45	357	187	236
4.	29·9	51	2668	2544	124	13	205	105	133

The greatest increase of azote was in the 2d and 3d experiments, when the thermometer was at 70°, which might materially influence the results: in the other cases, it was not higher than 53.

Capacity of the lungs,

From the experiments of Goodwin, we might be inclined to admit the capacity of the lungs, inferred from the 1st and 4th experiments, as very possible; but it seems difficult to conceive, that it can amount to 236 or 225 cubic inches, and yet this must be the case, unless a portion of azote is given off from the blood, or there is some process in nature by which it is capable of being produced from oxigen.

Having, by the kindness of our friend Henry Cline, jun., been furnished with the lungs of a stout man, about five feet ten inches high, taken from the body not long after death, and in a sound state, we proceeded to ascertain the quantity of air contained in this organ after the most complete expiration, as in death.

Henry

Henry Cline had judiciously taken the precaution to divide the trachea just below the cricoid cartilage, before he opened the thorax; he then inserted a tube with a brass stop-cock, which he tied firmly to the trachea, and attached an empty bladder to the other end. The cock was then turned, so as to communicate with the bladder, and on opening the thorax $31\frac{1}{2}$ cubic inches of air were expelled into it. The weight of the lungs was four pounds one ounce. A very large glass jar, being placed in a shallow tin vessel, was filled to the brim with water, the lungs were then completely immersed, and the water which flowed over, and was the measure of their volume, weighed six pounds two ounces: we next cut a portion of the lungs into small pieces, under a large inverted glass of water, and attempted to squeeze the air from the cells, but although several cubic inches were thus procured, we were soon convinced that it was utterly impossible to arrive at our object by these means, as no force that we could use seemed capable of expelling the air from the cellular membrane, into which it escaped from the vesicles. We therefore took portions of the lungs, which weighed 2774 grains; the mass, being put into a piece of new hair cloth, was subjected to the action of a powerful screw press, and the fluid was received into a vessel; after twice undergoing this operation, the mass weighed only 660 grains. Its specific gravity was very nearly that of water, viz. ·930, water being 1·000: the fluid procured by the press was of the specific gravity of 1·019; this would make the specific gravity of the lungs ·997, water being 1·000; hence it appears, that the substance of the lungs, and the contents of the blood-vessels together, are so near the specific gravity of water, that they may be fairly considered as the same.

examined in a stout man after death.

Then, as the mass of the lungs was equal to 4 pounds of water, though 6·2 pounds of water were displaced by them, and as a pound of water occupies the space of 28·875 cubic inches, we have the following calculation:

lbs.

lbs.	oz.	
6	2	water displaced by the lungs
4	1	weight of the lungs
2	1,	or 59·554 cub. in. of air in the lungs, to which must

be added ·31·580 the volume of the air forced into the bladder on opening the thorax.

91·134

and this gives us 91·134 cubic inches, as the air contained in the lungs of this person after death; and when we reflect, that the air must have been under compression, when the lungs were immersed in water, some force being required to keep them down, and also that not less than 7 or 8 cubic inches must be contained in the fauces &c., we cannot estimate the whole at less than 100 cubic inches.

It is farther to be noted, that these 100 cubic inches would occupy much more space in the temperature of the human body, than in the mean temperature in which the examination was made; and this difference would be nearly 8 cubic inches; the air left in the lungs, after complete expiration, would therefore be 108 cubic inches; but the mean of our experiments would make it 183.

Found to be 108 cub. inches.

Experiment	
1.	141
2.	225
3.	236
4.	133
	4)735
	183

Nitrogen evolved from the blood.

We are then almost compelled to allow, that, when pure oxigen is respired, a portion of azote is given off from the blood.

Experiment on a guineapig.

We now resolved to perform a series of experiments upon some animal, which lived wholly upon vegetable food, and made choice of the Guinea pig as one of the most manageable.

Apparatus described.

The apparatus consisted of two large mercurial gasometers, which were made to communicate with a strong trough

trough E, Pl. VII, in the middle of which a small mahogany table D was made fast by a screw, for the purpose of supporting the animal under the bell glass A. Two holes were made through the table for the insertion of tubes to supply and take off the air, each of them communicated with one of the mercurial gasometers; the tube B delivered gas towards the upper part of the glass A, in order to bring the supply of fresh air near the head of the animal: the opening of the tube C was placed within half an inch of the table to convey off the respired air; the gasometer connected with this tube was made to communicate with a mercurial bath G, in which portions of the respired air were preserved for examination. Quicksilver being poured into the trough E, so as to rise to a level with the top of the mahogany stand, we placed a Guinea pig upon it, with the bell-glass over him, and as its edges were immersed in quicksilver, the animal was completely confined in atmospheric air: we found that his body occupied the space of 39 cubic inches, which, deducted from the cubic contents of the glass A, left 55 cubic inches for the air confined with the pig, to which must be added 5 more for that contained in the tube C.

(To be concluded in our next.)

X.

On the Construction of Theatres, so as to render them secure against Fire. In a Letter from Mr. B. Cook.

To Mr. NICHOLSON.

SIR,

IN some former letters you did me the favour to insert in your Journal, I have recommended the use of iron in the place of wood; in my last on that subject I pointed out its use and advantages in substituting it for wood in buildings, but more particularly in adopting it for staircases, as promising a certain escape in case of fire. What I would direct the attention to in this is, the great advantage of employing

Iron recommended instead of wood,

particularly in public buildings.

ploying it almost entirely in the erection of public buildings, especially theatres; and although I am not an admirer and encourager of theatric representations, but, on the contrary, think they are injurious to a state, as contaminating the morals and habits of a people, and consider them as the very seat and emporium of vice and immorality, yet as they are permitted, it is a desirable thing, to have them erected in such a way, that, for safety's sake, the frequenters may not be in danger from any accident or other cause. We have seen the two national theatres destroyed entirely, and that with a rapidity that no human exertion could put a stop to. Their destruction arose no doubt from an unpardonable neglect, or a worse cause: one has risen like a phœnix from its ashes, more beautiful than before, but is it not risen with all the dangers of destruction in itself? It ought to have risen immortal; it ought, as a national establishment, to have been composed of such materials, as would mock a second dreadful devastation. A fire once commenced, would it not, in this new theatre, communicate to all its parts? would it not put at defiance the power of man to suppress it, and in a few hours would it not again be a heap of ruins? Drury Lane is still in this situation; and as it is in contemplation to raise it to its former splendour, and as another theatre is about to be erected also, I do hope, before they are erected, that the proprietors will carefully consider how absolutely necessary it is to compose them of such materials, as will endure for ages, and that cannot be again consumed with fire. The destruction of Covent Garden was accompanied with the loss of so many lives, that no care or expense can be too much to guard against so dreadful an accident. I mention not the loss the proprietors sustained, I take not that into the scale, for what is the loss of property, when compared with life? The nation ought to superintend the erection of its public buildings, especially those buildings set apart for amusement. The lives of his Majesty's subjects ought to be as carefully provided for by the legislature, in their meetings together for amusement, as it provides for them from their enemies from without. I always have considered all such places as extremely dangerous, not that I suppose that

Theatres conducive to immorality.

Two burnt down,

and one rebuilt,

but not secured against fire.

Others about to be erected.

The legislature should provide for the security of public buildings.

Danger from crowds trying to escape,

that a fire could begin and communicate itself round the house with such rapidity as to endanger the lives of the auditors; but what is as bad, or worse, on the first appearance or cry of fire, instantly would the audience rush from their places to the doors, and hundreds of lives would perhaps be lost; for the very idea of a fire deprives a man of that command over his reason, especially if he conceive the danger imminent, that at the first scream of fire, all would press to escape, so that numbers would be suffocated, crushed, or trodden to death. The second view I would take is, that on such immense piles of building being in a blaze, if the wind should be high, vast danger might be the consequence; and in the attempt to extinguish fires of this sort, we have a recent instance in Covent Garden, how dangerous is the employment of the firemen and assistants.

and in cases of high wind.

I will suppose, that a theatre was constructed of iron instead of wood. If the scenery during the midst of the performance was to take fire, and the whole in a blaze, the spectators might rest quiet, it could not extend to reach them; and I do think, that, although the whole house resounded with the cry of fire, the idea would fix itself on the mind so strongly, that they were sitting on iron, that the alarm would not so much affect them, if it did at all, as to produce any mischievous consequences. The very thought of the theatre being incombustible would draw to it many persons, who, from a fear of accident, might now keep away. The security it promises to the proprietors not only from real danger, but from the alarm of danger, would, I should suppose, (especially if it can be made appear, that it would be erected as cheap, or nearly so, as of wood) induce them to adopt this plan.

In a theatre where iron was used for wood a fire of no consequence.

I will give a brief description of the mode, and although imperfect, yet all I wish is to strike the mind with the idea, and induce the proprietors to give it due consideration. I will begin with the stage—all the upright and cross supporters should be iron, cast light, fitted, and screwed or pinned together, and to make it fire proof, projecting edges might be cast on the bottom of each spar, so that when laid down to lay the floor upon, tiles or quarries made thin and light on purpose might be laid between the spars, forming a solid

Plan for a theatre.

a solid fire proof bottom. Then the boards for the stage might be laid down and screwed to the spars; the same principle acted upon through all the rooms, and all the doors neatly made of iron, with pannels to fall into the mouldings neatly screwed in, which when painted would be as handsome as mahogany. The stairs and staircases all cast and fitted together in the way recommended in your Journal, No. 107, would be much more beautiful than it is possible to make wood, without going to a very great expense, and then not half so durable. They would be much cheaper than stone also. In fact, I would introduce little or no wood at all, except the floors, and these I would lay down fire proof.—It would then be impossible for fire to be communicated to the different parts of the house, was the vilest incendiary to gain admission under cover of the night, and fire the boards of the stage and scenery, there being none, or but little admission of air from below, owing to the fire proof bottom, it could make its way but very slowly, and then only the boards and scenery could be burnt. All communication being cut off with the adjoining rooms by the iron doors, it could not consume their contents, and the supporters of the floorings, being iron, could not conduct the flame. The front of the stage and orchestra should be iron; the orchestra in particular would be extremely handsome, cast with beautiful festoons of flowers or trophies, and painted in character. The flooring of the pit laid on arches could not be in danger, and the seats of the pit also should be iron, the supporters of the seats cast light, and the seats made in pannels from 3 to 6 feet, neatly cast to fall into the mouldings with round edges, so that when the supporters were fixed, the framing laid down, and screwed firm, then the pannels should be let into the mouldings, and fastened with screws also. The seats made in this way would be quite as neat, and all the objection I see is, that iron would be colder than wood; but when they are covered, that objection in part would vanish—at all events, the supporters of the seats might be iron, if the seats were wood. The framing for the boxes might all be of iron, and the seats iron also, for they are stuffed, therefore iron could be no objection. Then the partitions of the boxes might be very

Little or no wood, but the boards to form the floors, and these fireproof.

very neatly managed and light, being all cast in open work and the fronts of the boxes might also be cast in scrolls, Gothic, or in trophies, in fact, in any way, figure, or shape, fancy might invent, more tastefully, more light and elegant, than it is possible to do it in any other way. Then, if it was wished, the fronts of the boxes might be rendered warm, if the open work was objected to as cold, by lining them inside with cloth or velvet, which would form a very handsome ground, on which the scroll or fancy work of the boxes would be seen to great advantage; the back of the boxes and doors I would propose of iron laid in pannels, and tastefully painted or lined—a person would not be able to discover, whether they were made with wood or iron inside, if well managed in the padding. The lobbies, the small staircases, the railing, the supporters and framing of all kinds should be iron, and all the floors should be laid on fire proof floorings. The grand staircase might be made to have a beautiful effect, if a man of fancy and genius was to design it; and the whole of the grand entrance might be such, as would strike the beholder with amazement at its novelty, and the mind at the beauty and delicacy of its composition, as in iron the finest specimens of antiquity could be introduced. The supports of the galleries should be iron. The gallery floor laid down fire proof, and the seats, if not iron, should be on iron supporters—the gallery stairs and staircase all iron—and the roof should have no wood at all in it, but be composed of hollow or solid iron, cast light, and if each piece was graduated from bottom to top, would still make it more light, and yet be equally as strong. A roof of this description well secured together with screws and bolts, &c., would not be considerably heavier than wood, as the iron would possess strength equal to wood, at less than half the thickness.

Comparison of the advantages and disadvantages of iron & wood.

Now it would be well to compare the advantages iron promises above wood, before it is adopted; and in looking at its advantages, all its possible disadvantages should be coupled with them, in order to see it in its true point of view. One objection might be urged, that it would be difficult to erect a theatre or great public building on this plan, But I say there would be no difficulty in procuring

 men

men competent to undertake the erection of them, and who are able to plan, get made, and put together, all its several parts; and in those parts where there was a field open for genius and taste to shew itself, in the entrances, boxes, and other conspicuous parts of the house, the most beautiful and unique ornaments might be made, and finely cast in iron, and afterwards touched up with the tool, and painted to imitate whatever fancy might dictate. There would be, especially in a theatre, the greatest scope for genius; thus might be constructed the most elegant one in the world; and one that no accident, no misfortune, no incendiary could destroy—that would brave the utmost efforts of time to destroy it—that would endure for ages. Another disadvantage might be supposed, and that is the additional expense in the erection. This might be something more, especially if beauty was suffered to lead, and genius permitted to exercise itself in the ornamental part; but suppose it was to cost ten thousand pounds more, what is it, in a public national building? especially when you are certain in constructing it in this way, you are constructing a work that will endure for ages. If you use wood, how can you assure yourselves, but by some unforeseen accident it may meet with the fate of the other theatres? Money must not be put in the scale as a competent balance against security. After considering the subject, and examining the way Covent Garden is built and fitted up, I am more and more convinced in my mind, that iron might be substituted for all the timbers, and for fitting up a theatre complete in the way I have before hinted, as cheap, or nearly so, as though it was done with wood. It is then self evident, it would be more durable, perfectly safe from fire, and much more elegant, if raised under the auspices of a man of genius and good taste.

Expense.

I am, Sir,

Your obedient servant,

B. COOK.

Caroline Street,
Feb. 20, 1810.

REMARKS.

REMARKS.

Iron recommended by others.

That the construction of theatres is a matter of public concern has already been very justly observed by Mr. Edgeworth, in his paper on this subject, in vol. XXIII, p. 129; where too the use of iron and incombustible materials is strongly inculcated. Sir George Cayley likewise, in his paper, vol. XXII, p. 241, recommends the use of iron, but not to the same extent as Mr. Cook; though certainly the more of it can be employed the better. There is one thing however of which Mr. Cook does not seem to be aware, and that is the necessity for trap doors and openings in the floor that forms the stage: but this is of little consequence, for they might be contrived in a floor of iron, as well as in a floor of wood. To obviate the objection of increased expense, Mr. Cook brings against it the increase of duration, with the probability of larger audiences. But if we likewise take into consideration the saving of insurance, it would probably be found on calculation, that the use of iron would be by much the most economical. This saving, which does not appear to have been adverted to, probably from its amount not being generally known, will be considered as of no small importance, when it is understood, that the offices were paid forty shillings for every hundred pounds insured, previous to the burning down of Drury Lane theatre; and that, since this event, they will not insure at a less premium than four guineas per cent. Now the proprietors of the theatre lately erected at Covent Garden state the expense of erecting it at £150000; the insurance of which against fire would amount to no less than £6300 a year. If, as I suppose, scenery and dresses be not included in this estimate, the insurance would be still more to cover the whole. And to insure only one third of this, or £50000, as on the old theatre, the premium would be £2100 a year. Would not the saving such a sum annually more than repay the additional expenditure for rendering the building proof against fire by the general use of iron? to say nothing of other advantages. C.

Great saving on the score of insurance.

XI.

XI.

Letter from Mr. Prieur *to Mr.* Guyton-Morveau, *on the double Refraction of Crystals of Sulphate of Copper*[*]

Blue vitriol suspected from its crystallization to have a double refraction.

I Have long suspected, my dear kinsman, that sulphate of copper, from the form of its crystallization, must exhibit the phenomenon of a double refraction; but hitherto I have not been able to verify this conjecture, on account of the general want of transparency of its crystals. At length however I have succeeded in obtaining crystals sufficiently diaphanous for this observation, and found the double refraction very manifest.

This verified

Mode of examination.

I proceed in the known method, either viewing at some distance a pin between the crystal and the light, or looking at a luminous point produced by making a small hole in a card. But sometimes I render the effects much more evident, by looking at an object more or less distant through an opera-glass, between which and my eye I place the blue crystal: or, which appears to me in general the best method, I stand with my back to the window, and look at a very narrow slip of white paper, or a bright wire placed on a black ground.

A line joining the two images of a point observed with the crystal of blue vitriol is always in a direction nearly perpendicular to the edges of the prismatic surface of the crystal; and consequently it is requisite to hold these edges nearly parallel to the length of the pin or line we would see double, in order to obtain the greatest effect.

The crystals must be used before their surface is tarnished with that slight efflorescence, which they always acquire after being kept a certain time.

* Annales de Chimie, vol. LXV.

XII.

XII.

Table of the Rain, that fell in various Places in the Year 1809, *by the Rev.* J. Blanchard, *of Nottingham; with a Meteorological Table for the same Year, by Dr.* Clarke, *of that Town.*

RAIN TABLE, by the Rev. J. Blanchard, of Nottingham.

1809.	Chichester.	London.	Chatsworth, Derbyshire.	Horncastle, Lincolnshire.	Ferriby, Kingston upon Hull.	Heath, near Wakefield, Yorkshire	Manchester.	Lancaster.	Dalton, Lancashire.	West Bridgford, Notts.	Nottingham.
January	8·44	2·91	5·22	3·50	1·57	3·98	2·67	4·66	6·58	0·77	1·80
February	4·31	1·86	3·29	2·59	2·94	2·58	1·96	3·11	4·53	——	1·69
March	0·00	0·94	0·44	0·82	0·48	0·43	0·35	0·53	1·13	0·72	0·75
April	3·95	3·46	1·70	2·10	3·05	2·11	0·96	1·59	2·20	2·30	2·15
May	1·07	0·86	1·83	1·59	0·45	2·96	3·42	3·39	3·85	——	1·80
June	2·38	1·20	2·06	2·24	3·24	2·01	2·45	3·10	4·26	2·45	2·45
July	3·45	3·58	2·00	2·87	2·38	2·28	1·79	4·00	3·45	1·44	1·44
August	3·70	2·64	4·38	4·53	5·88	4·61	3·85	6·12	7·25	3·75	4·50
September ...	3·34	2·90	4·13	3·90	3·10	4·29	4·22	4·75	5·57	2·60	3·13
October	0·60	0·22	0·28	0·75	0·56	1·41	0.61	0·87	1·66	0·35	0·31
November ...	1·30	1·38	1·91	1·70	1·90	2·25	2·14	3·87	2·80	1·33	1·18
December ...	5·53	3·00	2·67	1·79	2·42	2·74	4·68	5·74	7·08	1·84	1·81
Total	38·07	24·95	29·91	28·38	27·97	31·65	29·10	41·73	50·36	17·55	23·01

METEOROLOGICAL TABLE,

By Dr. Clarke, of Nottingham.

1809.	Thermometer.				Barometer.				Weather		Winds.			
MONTH.	Maximum.	Minimum.	Medium.	Greatest Variation in 24 hours	Maximum	Minimum.	Medium.	Greatest Variation in 24 hours.	Fair.	Wet.	N. & N. E.	E. & S. E.	S. & S. W.	W. & N.W.
January..	5[illegible]	17	35·29	14	30·05	28·65	29·44	1·13	18	13	14	18	9	5
February	54	30	43 00	14	30·33	28·68	29·62	0·74	18	10	2	3	33	16
March ..	62	30	44 00	10	30·38	29·00	29·99	0·40	26	5	22	6	9	15
April....	56	28	42·76	13	30·36	28·97	29·73	0·54	13	17	16	1	14	18
May	77	38	57·61	9	30·26	29·23	29·84	0·43	24	7	10	13	19	13
June	74	45	57·71	18	30·45	29 27	29·84	0 62	21	9	11	7	15	14
July	78	46	59·64	10	30·12	29·39	29·88	0·24	17	14	24	4	10	18
August ..	76	48	60·69	8	29 97	29·23	29·64	0·45	12	19	3	8	31	8
September	72	34	50 46	12	29·87	29·05	29·46	0·63	25	5	2	5	11	17
October..	67	30	52·00	10	30·25	29·77	30·09	0·38	25	6	13	6	17	4
November	64	26	42·10	11	30·41	29·03	29·89	0·77	18	12	12	2	6	22
December	53	33	40·12	13	30·00	28·25	29·45	0·90	17	14	1	6	23	15
Total..									234	131	130	79	197	165

ANNUAL RESULTS.

Thermometer.		Wind.
Highest Observation, July 27th,	78°	S.
Lowest Observation, Jan. 22d,	17°	N.
Greatest Variation in 24 hours, June 1st-2d	18°	
Annual Mean	48·78	

Barometer.		Wind.
Highest Observ. June 25th,	30·45	N. E.
Lowest Observ. Dec 17th,	28·25	W.
Greatest Variation in 24 hours, Jan. 30th-31st	1·18	
Annual Mean	29·74	

Weather.	Days.	Winds.	Times.
Fair	234	N. & NE.	130
Wet	131	E. & SE	79
		S. & SW.	197
		W.& NW.	165
	365		571

Rain.	Inches.
Greatest Quantity in Aug.	4·50
Smallest ditto in Oct.	0·31
Total Quantity for the year	23·[illegible]

REMARKS.

REMARKS.

On the 22d of January, the thermometer within two miles of the town stood at 14°.—April 19th, snow had fallen to the depth of one foot.—May 2d. Snow fell this morning.—Aug. 6th. The rain that fell from 9 A. M. to 5 P. M. amounted to 1·72. Loud peals of thunder at noon, increased at 4 P. M., when the lightning became extremely vivid, the thunder tremendous, and the rain descending in torrents, and continued to do so most part of the night.—Dec. 17th. The barometer at 11 P. M. stood at 28·25. The following are comparative observations on the fall of the mercury:

Barometer.

	Keswick.	Kendal.	London.	Nottingham.
Jan. 1788	28·35	28·38	28·89	——
Jan. 1789	28·09	28·12	28·58	——
Dec. 1809	——	——	——	28·25

N. B. The barometer is firmly fixed to a standard wall over a staircase, on a level of 130 feet above the sea. The pluviameter is placed in a garden, on an elevation of 140 feet above the level of the sea.

XIII.

Large Fossile Cerite, found by J. C. DELAMETHERIE*.

Have a gigantic fossile cerite, cerites gigas of Lamarck, which Mr. Maclure, of the Philosophical Society of Philadelphia, and I found at Grignon, on a mineralogical tour we made in the month of July. It is the largest known. Its circumference near the mouth is two or three and twenty inches [23½ or 24½ E.] or about 7½ [8] in diameter. The thickness of the lip is about 7 lines [6 E.]. Its whole length must have been about 30 inches [32], but the piece I have is broken, and not above 10 or 12 [10½ or 12¾] inches long.

Large fossil murex.

* Journal de Physique, vol. LXV, p. 411.

XIV.

XIV.

Reflections on the Species of Moss proposed as a substitute for Wool in stuffing Beds, Furniture, and Garments, by Mr. PARMENTIER*.

Moss proposed as a substitute for wool.

THE dearness of wool, and more particularly the property it has of imbibing putrid miasmata, and propagating contagious disorders, suggested the idea of supplying its place in beds by the *hypnum crispum L.* a kind of moss of a moderate length, and of a somewhat fragrant smell. Mr. Isengard, inspector of direct contributions at Savonne, has sent to the Society of Encouragement a specimen of this moss, taken from a mattress that has been in use for some years, with a paper in which he relates the methods of preparing it for domestic purposes.

Examined by the Society of Encouragement.

This Society, which attends to every project offered it, if its object be of public utility, and to promote our home manufactures, submitted it to the examination of its committee of economical arts; and Mr. Bouriat, one of its members, drew up a report, in which the advantages and inconveniencies, that might arise from the proposal, are justly estimated. The judicious observations in this report leave nothing to be added to them.

One of our most intelligent army physicians too, Mr. Michel, has communicated to us his ideas with respect to the hypnum crispum, both medically and economically. The following is an extract from his letter.

Bedding of the sick infectious.

"A great number of patients labouring under fevers, all of the nervous kind, in the French hospital at Genoa, where I was employed, convinced me, that the bedding of the sick is of itself sufficient, to render the slightest disorder complicated; as it is generally acknowledged, that wool imbibes contagion, and propagates it, which dangerous properties vegetable substances happily do not possess."

Moss recommended.

Among those exempt from this inconvenience he particularizes this moss, which he met with in Italy in every wood, particularly on beech trees. It is gathered in August and September,

* Abridged from the Annales de Chimie, vol. LXV, p. 175.

September; beaten like flocks, does not retain moisture, or form into lumps like them; is little liable to decay; and costs only the price of the labour, so that four mattresses made with this moss will cost less than one of wool. It is only necessary to dry it in the shade, to preserve its fragrance.

Its advantages.

Mr. Michel observes, that neither sweat nor urine produces any fermentation in this moss, as it does in wool: but, lest moisture should occasion it to germinate, he recommends steeping it in limewater, which destroys its power of vegetation.

The idea of employing moss for this purpose, observes Mr. Parmentier, is by no means new; for, if we may trust the poets, it was common among the ancients. It is a mistake, however, that vegetable substances cannot propagate contagion, for cotton is notoriously in bad repute on this account: and if we were to have recourse to the mosses to supply the place of wool in stuffing articles of furniture, as there are but few species adapted to the purpose, we should soon be at a loss for an adequate supply.

Not superior to wool.

SCIENTIFIC NEWS.

R. von Humboldt informs us, that between Valladolid, in New Spain, and the lake of Cuisco, which is impregnated with muriate of soda, in a space of forty square leagues, there are a number of warm springs, the water of most of which contains nothing but muriatic acid, without any metallic salt.

Native muriatic acid.

The presence of fossil shells in gypsum is so rare, that several naturalists have questioned the fact. Messrs. Cuvier and Brongniart, in their first paper on the geographical mineralogy of the environs of Paris, say, that freshwater shells have been found in the upper strata of gypsum, and the marles immediately upon them; but they say nothing

Sea shells in gypsum.

thing of sea shells in any of the strata below the first beds of gypsum. Mr. Desmarest, of the Institute, is the only person who had mentioned turbines and other fragments of shells in the last beds of the third mass, which is the deepest; and their presence has lately been confirmed by his son and Mr. Prevost. The first fragments of these shells are discovered below what is called the great bed of gypsum. The second, consisting of more species than the former, is in a calcareous marle below what is called the little bed. All the species to which these shells belong are found at Grignon. They are calyptræ, murices, cerites, turritellæ, volutes, ampullariæ, cockles, tellinæ, cithereæ, solens, corbulæ, &c. Beside these are fouud glossopetræ, vertebræ of fishes, claws and shells of crabs, echini of the genus spatangus, and consequently different from those of Grignon which belong to the cypeastræ. Three small beds of gypsum, and some strata of marle succeed, without any shells: and lastly we come to a bed of calcareous marle, in the midst of which is a bed of gypsum. Both these contain the same shells. They are ceritæ, which we may refer to the genera petricolum and tetebrale. In both the shell itself has disappeared; but in the first marle we find the figure of its surface in relief, and the interior filled with marle; and in the second marle and the gypsum there is a hollow moulded by the outer surface of the shell, and a nucleus moulded by the inside of the shell, while the space occupied by the shell itself is empty. Thus we have sea shells perfectly similar to those of coarse limestone, not only in marle lying between beds of gypsum, but in the gypsum itself.

Marle assuming regular figures.

Messrs. Prevost and Desmarets have made another observation on the stratum of marle, that lies below the little bed of gypsum, and which contains shells. They have found this marle in certain spots affecting the shape of quadrangular pyramids, the faces of which are striated parallel to the edges of the base. They noticed more than twenty of these pyramids, some of which are as much as 3 cent. [1·18 inch] high, with a square base of 8 cent. [3·15 inches] each way. These pyramids cannot be considered as halves of octaedra; for their base is so confounded with the marle, that the opposite faces for completing the octaedra are by no

no means discoverable. But they have this singularity in their arrangement, they are always found in groupes of six, with their faces touching each other, and their summits meeting in one point. The union of these pyramids produces a cube, though its sides are not perceivable, because the bases of the pyramids are continued uninterruptedly into the marle, which is precisely of the same nature as the substance of which they are formed.

Plantations of forest trees.

In the course of seven months, from october 1805 to april 1806, Mr. Curwen, of Workington hall, in Cumberland, planted on 400 acres of land upwards of 1208000 forest trees, for which he was adjudged the gold medal of the Society of Arts. He likewise planted 96100 trees in other places within the same period. Nearly half of the whole number consisted of larches.

A gold medal was likewise voted to Dr. W. Thackeray, of Chester, for having planted on the estate of his stepson and ward, between october 1804 and june 1808, on 171 acres of land, 1133759 forest and 114 fruit trees.

Plantation of oaks.

Mr. Congreve, of Aldermaston house, in Berkshire, has planted, since the beginning of 1802, upwards of 74 acres of land with oaks, for which he received the premium of the gold medal.

Map of Shropshire.

Mr. Robert Baugh, of Llanymynech, in Shropshire, has received the silver medal of the Society of Arts, and 15 guineas, as a premium for his nine-sheet map of Shropshire, published last year from an actual survey.

Double brocade weaving.

In order to confute the idea, that the silk-weavers of this country could not produce manufactures equal to the French, a society was formed some time ago, termed the Flag Association, with a view to the production of such a specimen of double brocade weaving as had never before been attempted. In consequence there is now in the loom a flag two yards wide, the ground a rich crimson satin on both sides, and brocaded on each side alike, with appropriate colours tastefully and elegantly shaded by the artist.

Upon its surface will appear woven within an oval, a female figure emblematic of the art of Weaving, reclining with pensive aspect on a remnant of brocade, lamenting the neglected state of this manufacture. A figure of Enterprise is

is represented in the generous act of raising her up, and reviving her drooping spirits, by showing her a cornucopia pouring forth its treasure, emblematical of the resources of this happy isle, and not unaptly indicating, that the wealth and liberality of the British nation are ever ready to support laudable undertakings, and particularly those intended for the relief of indigent merit. Close to that of Enterprise, and under a representation of the all-seeing eye of Divine Providence, the figure of Genius appears erect, pointing to a flag displaying *the Weavers' Arms*, placed upon the temple of Fame, seeming by her expressive countenance to say, "execute your arduous task, Britannia will reward your labours, and Fame, inscribing them on her sacred edifice, shall record the merits of this grand exertion to posterity." The corners of the flag will be adorned with emblems of peace, industry, and commerce. An edging with a curious Egyptian border will exhibit a combination of figures and devices, emblematic of the design for which it was formed, and the whole will in an expressive manner show to the world this interesting fact, that under the auspices of Divine Providence, and cherished by the blessings of peace and commerce, the British artists, when fostered and protected, are inferior to none throughout the globe.

The Society of Arts have presented their silver medal, set in a broad gold border, to the Flag Association.

Iron chains used instead of ropes in mining.

Mr. Gilpin has made an improvement in the pulleys in which his iron chains work (see Journal, vol. XXI, p. 111.) Finding a small degree of brightness in the links that work vertically, he made a concave rim to the pulleys on each side, to embrace the links that work flat, and prevent the others from coming into contact with the sides of the grooves. In this state the chain works 7·68 per cent easier than before; 28·87 per cent easier than a half worn, strand laid, tarred rope; and 37·51 per cent than the same chain used in the common way. Upwards of six years experience confirm his former opinion of the safety, durability, and cheapness of chains worked in grooves; yet, he observes, his men will not go down into the pit to work in the morning, or come up at night, but by a rope; though, if any scene of amusement be going forward on the surface, they will not hesitate

hesitate to come up two or three together riding on the loaded basket that is drawn up by the chain.

Black pepper, in the West Indies,

The black pepper plant thrives remarkably well in the botanic garden at St. Vincent, under the care of Dr. Alexander Anderson, and has been producing fruit there some time. The doctor finds it a plant of more easy cultivation than he had imagined. He has likewise collected a considerable quantity of cloves.

and cloves.

Parkinson's Introduction and Organic Remains.

Mr. Parkinson has withdrawn the *Introduction to the Knowledge of Fossils,* announced at the end of his first volume of *Organic Remains of a former World,* considering its publication as entirely superseded by Mr. Martin's excellent systematic *Outlines* of the same subject.

The *third volume* of *Organic Remains* is in considerable forwardness.

Wernerian Natural History Society.

Mineralogy of the Highlands.

At the meeting on the 3d of February, the Rev. Dr. Macknight read to the Society an account of the mineralogy of the Highlands of Scotland, from the Pass of Leny to Balahelish; which he illustrated by specimens. The general rock in this tract is mica-slate, with its usual subordinate beds, such as, of granular limestone, hornblende-slate, &c. It contains also in some districts, beds and veins of lead-glance, and indications of ironglance. Beyond Tyndrum, the mica-slate approaches to gneiss, till we pass Inverouran, where sienite appears. In the neighbourhood of King's House, newer granite, felspar, porphyry, and hornstone are found; and the adjacent country, as might be expected from the decomposition of these rocks, presents, for many miles, an unusual aspect of bleakness and sterility. Glencoe, which is singularly interesting, both in a picturesque and in a mineralogical point of view, consists of hornstone and compact felspar, in beds subordinate to the primitive rocks, and capped with porphyry. At the bottom of Glencoe, mica-slate again appears, and is covered with the formation of

clay-slate

clay-slate, which affords the well known roof-slate quarries of Balahelish. Thus, according to Dr. Macknight, it appears that the relative positions of the great formations, which occur in the Highlands of Scotland, correspond to the principles of the Geognosy of Werner.

Universality of rock and metalliferous formations.

At the same meeting, Professor Jameson read some observations on the universality of rock and metalliferous formations, preliminary to a short account of some specimens of a particular formation of lead ore found within fifteen miles of Dunkeld in Perthshire. The formation appeared to be almost the same with that which occurs at Strontian in Argyleshire; and it is therefore not improbable, that it may prove a source of wealth to the proprietor.

Natural history of the whale.

At this meeting, likewise, the Secretary read some new and interesting observations on the natural history of the common Greenland whale, by Mr. William Scoresby, jun., of Whitby; and exhibited a correct drawing of that animal, by the same gentleman, differing materially from the representations generally seen in books.

TO CORRESPONDENTS.

The principle in hydraulics, which my correspondent who has favoured me with a sketch of a double inverted vessel or diving bell alludes to, does not appear to be capable of producing the effect he expects from it. If I understand the description of his apparatus, the result would be, first, that air would be driven up the tube, and from beneath the upper vessel, until the lower surface of the water had risen and filled the tube; after which the escape of air would continue from the upper vessel, until the surface in both vessels were on the same level, and at this period the included air would act against the water precisely in the same manner, and with no greater advantage, than in the common diving bell.

W. N.

ERRATA.

Page,	line.	
95,	6 from bot.	The sentence beginning here, and ending line 4 from bottom, was intended to be in the form of a query: "Will not this plant perform its motions under water?"
162,	4,	*For* 50°·40′, *read* 50° 40′.
	11,	*After* one, *add* in respect to the convenience of the movement.
164,	10 from bot.	*For* quality, *read* quantity.
169,	15 from bot.	—— lightness, —— tightness.
170,	8 & 9 from bot.	—— unanimously, —— exceedingly.
171,	9 from bot.	—— lamina, —— laminæ.
187,	16,	—— 4′ —— 4‴.
190,	13 from bot.	—— 4oz, & ½oz. —— 4 drachms, & ½ a drachm.
238,	24,	—— alumine, —— aplome.

Errata in Vol. XXIV.

Page,	line.		
168,	4,	*For* complete,	*read* complicate
	8,	—— *e d*	—— *c d.*
	19,	—— *b d e*	—— *b d* c.
170,	23,	—— 27·3	—— 37·3
171,	13,	—— 94	—— 84

METEOROLOGICAL JOURNAL,

For MARCH, 1810,

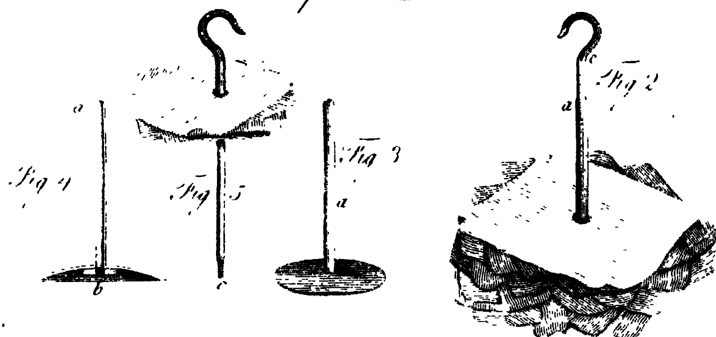
Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON,

FEB. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
27	50°5	48°	53°	46°	29·73	Cloudy	Cloudy
28	49·5	49	52°5	47	30·02	Fair	Ditto
MAR.							
1	50	51	53	48	29·88	Fair	Cloudy
2	50	49	52·5	37	29·70	Rain	Rain
3	40	46	52·5	38.5	29·61	Ditto	Fair
4	40	45	48	39	29·68	Ditto	Rain
5	41·5	41	44	34	29·27	Cloudy	Cloudy
6	35	41·5	42	40	28·92	Snow	Ditto
7	42·5	43	45	39	28·92	Rain	Rain
8	46	46	49	45	28·96	Ditto	Ditto
9	52	52	55	48·5	29·17	Ditto	Ditto
10	52·5	50·5	56·5	46	29·45	Fair	Fair
11	51	52	55	49	29·94	Rain	Rain
12	52	47	54	39	29·68	Ditto	Fair
13	41	41	43.	37	30 03	Ditto	Ditto
14	40	36	43	33·5	30·01	Cloudy	Ditto
15	37	36	39·5	33	29·81	Fair	Cloudy
16	36	36	40	35	29·54	Hail	Ditto
17	38	34	41·5	30	29·67	Fair	Fair
18	36	35	43	31	29·88	Ditto	Ditto
19	33·5	42	44	31·5	30·02	Ditto	Ditto
20	39	40	42·5	33·5	30·05	Ditto	Ditto
21	41	46	48·5	35·5	29·76	Ditto	Ditto
22	39	37	43·5	31	29·97	Ditto	Ditto
23	36·5	42·5	48.5	36	29·88	Ditto	Ditto
24	39	40	48	34·5	29·76	Ditto	Cloudy
25	38	39	41	36·5	29·80	Ditto	Ditto
26	38	39·5	44	36	29·93	Ditto	Fair
27	45·5	45	53	41	29·76	Rain	Cloudy

Lieut. Spratt's Homograph



Mr. White's improved Letter File.



J. Gough Esqr. on some Properties of Solids

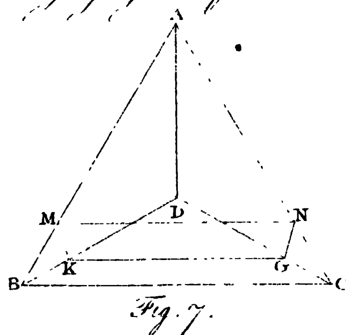


Fig. 7.

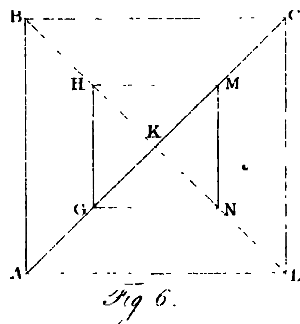


Fig 6.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XXV.

ARTICLE I.

An Inquiry, Geometrical and Arithmetical, into certain Properties of Solids in general, and of the five regular Bodies in particular. By JOHN GOUGH, *Esq.*

DEFINITIONS. 1*st*. AN edge of a solid is a line terminated by two of its solid angles, and common to two of its adjoining faces. Def. 1.

Def. 2*nd.* From *Pappus lib.* 5. Ordinate plain figures or polygons are those which are bounded by equal right lines, containing equal angles. Def 2.

Def. 3*rd.* Two or more plain figures are said to be of the same denomination, when each and all of them are bounded by the same number of right lines. Def 3.

AXIOMS. *Ax.* 1*st*. Three plain surfaces cannot contain a solid. Axiom 1.

Cor. Hence a pyramid under four triangles is the simplest solid bounded by planes. Corollary.

Ax. 2*nd.* Any required solid may be cut out of a given solid. Axiom 2.

PROPOSITIONS. *Prop.* 1*st*. Let c, t, s, be the number of the faces, solid angles, and edges of a solid, bounded by Prop. 1.

 polygons

polygons of the denomination n; and let p denote the number of these polygons, which bound each solid angle of the body; then we have $s=\frac{n\,c}{2}=\frac{p\,t}{2}$. For there are c faces and each face has n edges (*Def. 3rd.*); but each edge is common to two faces (*Def. 1.*); therefore $s=\frac{n\,c}{2}$. In like manner there are t solid angles, and each solid angle is bounded by p faces, and therefore is touched by p edges; hence $s=\frac{p\,t}{2}$. Q. E D.

Demonstration

Cor. 1.

Cor. 1*st.* $n\,c=p\,t$, and $c=\frac{p\,t}{n}$; $t=\frac{n\,c}{p}$.

Cor. 2.

Cor. 2*nd.* Now let the given solid be a pyramid under four triangles; and $n=3$; $c=4$; $p=3$; because it cannot be greater or less than 3; hence (*by Prop.*) $s=3\times 4\div 2=6$; and by (*Cor.* 1.) $t=3\times 4\div 3=4$; therefore in this case $s=c+t-2$.

Prop. 2.

Prop. 2*nd.* If a solid angle K (Plate VIII, Fig. 6,) be contained under p planes, *viz.* A K B, B K C, C K D, D K A, &c.; and the solid angle K be cut off by any plane G H M N; the section will be a plane bounded by p right lines and p plain angles. For G H M N cuts each of the planes A K B, B K C, &c.; because it cuts off the solid angle K; but its intersections with these planes, *viz.* G H, H M, M N, N G, &c., are right lines (Euclid 3.11), and any two of them are in the same plane (Euclid 2.11); therefore they are all in the same plane, and their number $=p=$ the number of planes containing the solid angle K; but the number of plane angles G, H, M, N = the number of sides G H, H M, &c. $=p$. Q. E. D.

Demonstr.

Corol.

Cor. If the solid angle K be cut off by a curved surface, the section will be a curved surface, bounded by p curves and p angles; and this will happen, if all or any of the faces A K B, &c., be curved.

Prop 3.

Prop. 3*rd.* Let A B C D K, Fig. 6, be a solid bounded by surfaces of any kind, and let c, t, s, be the numbers of its faces, solid angles, and edges; and let one of its solid angles, K, be cut off by a surface of any kind, G H M N; the increment of c together with the increment of t = the increment

ment of s. For since one solid angle K has been cut off, one surface G H M N has been added to the number c (by *Prop.* 2, and its *Cor.*); therefore the increment of $c=1$; but G H M N touches p solid angles with its angles G, H, M, N, by the same, and one solid angle (K) has been cut off; therefore the increment of $t=p-1$; and the increment of c + the increment of $t=p$: but no edge has been cut off; therefore the number of the edges G H, H M, M N, N G, has been added to s: but this number $=p$ also (by *Prop.* 2*nd.*) Q. E. D.

Demonstr.

Prop. 4*th.* If a solid of any kind, A B C D (*Fig.* 7.), have any number of its solid angles, as B, C, cut off by a surface of any kind, M K G N, so as to change the number of its faces; the increment of c together with the increment of t = the increment of s. For, put f = number of lines and angles bounding the additional face M K G N; d = the number of solid angles, B, C, &c., cut off: then the remaining solid, A M K D G N, has one surface more than the solid A B C D; hence the increment of $c=1$; but the surface M K G N touches f new solid angles in M, K, G, N; and d solid angles have been cut away at B, C, &c.; therefore the increment of c + the increment of $t=f+1-d$. Again, the additional edges of the solid A M K D G N are f in number, being the lines bounding the face M K D N; but when two solid angles are cut away, as B, C, one edge, B C, is lost; and in general when d solid angles are cut away, so as to increase the number of faces belonging to the new solid, A M K D G N, $d-1$ edges are lost; hence the increment of $s=f+1-d$. Q. E. D.

Prop. 4

Demonstr

Prop. 5*th.* Let c, t, s, denote the number of faces, solid angles, and edges of any solid whatever, and we have the following general expression; $c+t-2=s$. For let the solid A M K D G N be cut by one operation from the tetraedron A B C D (Fig. 7), or the solid A B C D G H M N (Fig. 6.) be formed by repeated operations from a similar figure; both of which suppositions are possible (by *Ax.* 2*nd*): then the sum of the additions made to c and t by one or several operations is equal to the addition or additions made to s in the same manner; therefore the difference of $c+t$ and s in the tetraedron is equal to the same difference in

Prop 5

Demonstr

the solid A B C D G H M N, or any other solid; that is $c+t-2=s$, (by *Cor.* 2, *Prop.* 1.) Q. E. D.

Prop 6.

Prop. 6th. Let A B C D (Fig. 6) be a solid bounded by polygons of the same denomination; put $n =$ the number of sides of each polygon: $p =$ the number of polygons which touch each solid angle: then $c = 4\,p \div 2\,p + 2\,n - n\,p$; $t = 4\,n \div 2\,p + 2\,n - n\,p$; $s = 2\,n\,p \div 2\,p + 2\,n - n\,p$.

Demonstr.

For $s = n\,c \div 2 = p\,t \div 2$ (*Prop.* 1*st.*) $= c + t - 2$ (*Prop.* 5*th.*); and $t = n\,c \div p\,c = p\,t \div n$ (*Cor.* 1, *Prop.* 1.); therefore $n\,c \div 2 = c + n\,c \div p - 2$; hence $c = 4\,p \div 2\,p + 2\,n - n\,p$: again $p\,t \div 2 = p\,t \div n \times t - 2$; and $t = 4\,n \div 2\,p \times 2\,n - n\,p$; again $p\,t \div 2 = p\,t \div n + t - 2$; and $t = 4\,n \div 2\,p + 2\,n - n\,p$: but $s = c + t - 2 = 2\,n\,p \div 2\,p + 2\,n - n\,p$. Q. E. D

Corol 1

Cor. 1st. p, which is constant relative to any given solid, denotes the number of polygons, which surround each solid angle of that body; therefore the plain angles of these polygons are equal among themselves; that is, the polygons are ordinate figures (by *Def. 2nd.*), and the preceding proposition relates to the regular bodies only.

Corol 2.

Cor. 2nd. If there be two regular bodies, one of which is bounded by ordinate polygons of the denomination n, each solid angle of it being contained under p planes, and the other is bounded by polygons of the denomination p; each solid angle of it being contained under n planes; c in the former $= t$ in the latter; and c in the latter $= t$ in the former; but s is the same in both. This is evident from the equations in the proposition.

Corol 3

Cor. 3rd. $2\,p+2\,n-n\,p$, the common divisor of the three preceding equations, is equal to one, or greater than one; but n is at least $= 3$ (Euclid, Ax. 10, Bk. 1); also p is at least $= 3$ (Def. 9, Bk. 11). Now to find the limit of n, put $2\,n+2\,p-n\,p=0$; and $p=2+4 \div n-2=a$, w, n, which gives $n=6$; therefore n can only be, 3, 4, or [illegible] that is, the regular bodies are bounded by triangles, [illegible] and pentagons [illegible]ly.

Corol 4.

Cor. [illegible] Put $n=3$ (and by prop) $t=$ [illegible] hence $p=3$, [illegible] 5; put $p=3$; and $c=4$; $t=$ [illegible] properties of a tetraedron; put $n=4$; and $t=$ [illegible] p; $p=3$; $c=6$; $t=8$; now put $n=3$, [illegible] $t=6$; the former of these is the cube; and, the latter [illegible] dron; and $s=12$ in both cases (by Cor, 2.) [illegible] 6,

and

and $t = 20 \div 10 - 3p$; hence $p = 3$; $c = 12$; $t = 20$; put $p = 3$; $n = 5$; then $c = 20$; $t = 12$ by *Cor.* 2; the former of which is the dodecaedron, the latter the icosaedron; and $s = 30$ in both (by Cor. 2.)

Prop. 7.

Prop. 7*th.* The sum of the plane angles bounding the solid angles of a regular body $= 8np - 16p \div 2p + 2n - np$.

Demonstr.

For the sum of the plane angles belonging to each face of the body $= 2n - 4$ (Euclid 32.1); but there are c faces; therefore the whole sum $= \overline{2n-4} \times c$; and $c = 4p \div 2p + 2n - np$; therefore &c. Q. E. D.

II.

Invention of a Homograph, or Method of Communication by Signals, on Sea or Land. By LIEUTENANT JAMES SPRATT, *of the Royal Navy**.

SIR,

Anecdote of the battle of Trafalgar.

WITH this you will receive a truly ingenious invention of Lieut James Spratt, of the Royal Navy. This gallant officer, in the glorious action of the combined fleet at Trafalgar, on the 21st of October, 1805, was on board his Majesty's ship Defiance. When engaged within pistol shot with a French eighty-gun ship called l'Aigle, he plunged into the sea, swam to the enemy's stern, and entered the gun-room port alone, made his way courageously through the different decks, and succeeded in mounting the enemy's poop, where placing his hat on the point of his cutlass, he called out to his men to join him. In attempting to haul down the French colours, he was attacked by several of their grenadiers, whom he repulsed with success. He was soon followed by several of our jolly tars, and in the act of saving the life of a French officer who cried out for quarter, a musket was levelled by a Frenchman at his own breast, which fortunately struck downwards, but his leg was

* [illegible] Society of Arts, vol. xxvii, p 163. The sil[illegible] Lieut. Spratt for this invention.

fractured

fractured by the shot; he afterwards fought two of the enemy on his knees, who were quickly dispatched by his companions, and the French ship soon after struck. More particulars of this transaction are recorded in the XV volume of the Naval Chronicle, page 193. I have the pleasure to add, that Lieut. Spratt, after a tedious illness, has recovered the use of his leg, and now has the command at the signal post at Teignmouth, anxiously wishing to be again employed in more active service against the enemies of his country.

I have taken the liberty of sending this communication, and the account of his invention, unknown to him, knowing that the Society of Arts &c is generally disposed to encourage merit in every rank and situation wherever found.

M. S.

Description of a simple and ready telegraph

This new, easy, and useful code of signals is to be performed with a white pocket-handkerchief, to be held in different positions with the body. Plate VIII, figure 1, A, with the dotted lines, exhibits the whole of the numeral homograph signals at one view, (see the positions that the handkerchief is held in, and the figures marked). The first position from the right foot to the right hand is No. 1, the others No 2, 3, 4, 5, 6, 7, 8, 9, and 0, follow in succession. When making 1, 5, 9, and 0, the handkerchief should be held by the diagonal corners, as generally prepared for wearing round the neck.

For making 2, 3, 4, 6, 7, 8, the opposite sides of the handkerchief should be gathered in each hand, the near extremity of the handkerchief to be held by one hand to the point of the shoulder.

In working the homograph the body should be erect, the positions steady, the handkerchief to be held well in front of the arms, and facing the person to whom you are to impart your intentions.

Best situation for using it in a ship,

The best place for showing signals from a ship is in the chains, or on a lower deck port, as the white handkerchief exhibits a greater contrast with the black sides, and is of course better discerned.

or on shore

When on shore, they should be made from the side of a green hill, or in front of some thick foliage, or hedge, or dark wall.

The

The positions intended to compose the number of signals should be made in succession. The person, to whom a signal is made, should wave his handkerchief horizontally, to convince you that it is understood.

Method of using it.

When the positions which compose the number of your signals are finished, you are to wave your handkerchief in like manner. For example, persons who make use of the homograph should arrange in their separate books, or from a telegraphic dictionary, every question and answer, which may occur to them on any subject, as there is no limitation to the numbers. If the number affixed to your communication be 1000, you are first to make position No. 1, and keep it so until your consort answers it by waving his handkerchief, which informs you that it is understood; then you are to make the 0 three times distinctly, as shown at figure A, each 0 to be kept up until answered as before. Now your signal being made, wave your handkerchief, which informs your consort, that he is to refer to his book for the purport of the signal No. 1000, which may either refer to a distinct word in dictionaries numbered alike, or to a whole sentence in conversations, premeditated and inserted in books formed for the purpose.

When you wish to commence a communication by signals, you are to display the handkerchief in the manner shown at fig. B, which is called the signal of attention, and your consort is to display his in return. The person who displays first has a right to begin the communication, and to prevent confusion, it is to be displayed at the commencement of every signal.

If by any accident your attention should be called off, and you did not comprehend the whole of a signal, by holding the handkerchief as in fig. C, you may demand a repetition. This signal is called the repeat. Fig. D, shows the following signals, by twisting the handkerchief regularly round one of the arms, and holding it in one of the positions marked, 1, 2, 3, 4, viz. No. 1, affirmative; No. 2, [illegible] No. 3, interrogative; No. 4 to annul.

SIR,

SIR,

Date of the invention.

I ACKNOWLEDGE the receipt of your letter respecting my homograph, and beg leave to express the high sense I entertain of its having been noticed by so distinguished an institution as that of the Society of Arts, &c. I am positively the first inventor of it, and I put it in execution at the commencement of this war, on board his Majesty's ship Defiance, commanded by P. C. Durham, who did me the honour to express his approbation of it, and promised to bring it to the notice of his Royal Highness the Duke of York; but having the honour of getting a fractured leg in the battle of Trafalgar, I was prevented from getting attention to my homograph.

Visible at the distance of 4 miles.

I have frequently conversed in this manner with my messmates at Spithead, from the green ramparts at Portsmouth, and from Plymouth Sound to the Hoe, which is still a greater distance. The conversation may be carried on at the distance of four miles by a common telescope.

Its various uses

The various uses to which the homograph may be applied at any moment, without expense, will not fail to attract the notice of persons of discernment. In active military service it will be found very important. It will be found useful to naval captains lodging on shore, who may thus communicate any orders with ease and accuracy to the commanding officers of their ships at anchor. Passengers on board ships in fleets may keep up a constant and friendly intercourse, to console themselves for the tediousness of a long voyage; and the country gentleman may, at a moment's warning, summon his neighbours to the sports of the field, or to the hospitable board.

If my homograph should meet with the approbation of the Society of Arts &c., and tend to the good of the community at large, I shall be highly gratified.

I have the honour to be,

With respect and esteem,

Sir,

Your humble servant,

[illegible]

Teignmouth Signal-Post,
Dec. 26th, 1808.

The

The following Certificates were received from Lieut. Glanville, R. N.; Lieut. Tayler, R. N.; Mrs. Spratt, of Burlington-House; and Miss Taylers, daughters of the mayor of Devizes.

Certificates.

I do hereby certify, that being at Ryde in the Isle of Wight in the summer of the year 1806, I frequently conversed with Lieut. Spratt, by means of the homograph invented by him; and previous to the battle of Trafalgar, Lieut. Spratt conversed in this way with many officers at Gibraltar. I also certify, that I have heard from officers of the Brest, Cadiz, and Mediterranean fleets, that Lieut. Spratt was the inventor, and the first person who made use of such invention. Given under my hand this 10th day of Jan. 1809.

J. N. TAYLER,
Lieut. of his Majesty's ship Spencer.

We the undersigned do hereby certify, that being at Ryde in the Isle of Wight in the summer of the year 1806, we did frequently see Lieut. Spratt of the Royal Navy conversing from the shore, by means of the homograph invented by him, with Lieut. J. N. Tayler, then on board his Majesty's ship the Leopard, lying at Spithead. Ann Tayler further saith, that she with many others saw it used with great success at Teignmouth.

Witness our hands, Feb. 2d, 1809.

MARY TAYLER, } Devizes,
ANN TAYLER, } Wilts.
MARGARET SPRATT. }

This is to certify, that I, George Glanville, Lieut. of the Royal Navy, saw Lieut. Spratt, then master's mate of the [illegible]ce, conversing by means of a homograph from [illegible]fiance, with Lieut. Nicholas on board the Malta, [illegible] months previous to the Trafalgar action, and that [illegible]med perfectly to understand each other by the [illegible]

Witness my hand this 30th day of May, 1809,
[illegible]ORGE GLANVILLE, Lieut. R. N.

III. *Description*

III.

Description of an improved File for Receipts and Letters. By Richard White, *Esq., of Essex-Street**.

SIR,

Inconveniences of the common file, for letters and receipts.

I SEND herewith the model of a file for papers, which I think will be found preferable to any in common use. A voucher cannot be disengaged from the common file, without defacing it by cutting it off, or by removing many others to get at it; and to return it to its proper place is attended with more trouble and inconvenience. All this is avoided by the contrivance in the file now sent, the wire of which is passed through a cylinder, and fastened by a screw at the bottom.

Avoided in a new one

I am, Sir,
Your very humble servant,
RICHARD WHITE.

Explanation of Mr. White's improved Letter File, as shown in Plate VIII, *Figs.* 2, 3, 4, 5.

This file described

This invention consists of a metal tube *a*, fig. 3, with a convex circular plate soldered to its lower end, to keep the papers from slipping off the file, and having attached to its under side a piece of metal *b*, fig. 4, with a screwed hole in it, to receive a screw on the end of the wire *c*, fig. 5, the other end of the wire being formed into a hook, sharpened at its point, to receive the papers as usual.

Method of using it.

When any paper is wanted to be taken off the file, (instead of taking off those above it, which cannot be replaced again without much loss of time and trouble, or, which is still worse, tearing it off) the uppermost papers are to be slipped up towards the top of the wire *c*, which must be unscrewed, and, with the papers upon it, removed, as shown in fig. 5; the paper wanted may then be taken away, the wire replaced again into its tube *a*, and screwed fast, and the other papers drawn down the tube as before. The up-

* Transact. of the Society of Arts, vol. [illegible], p. [illegible]. The silver medal was voted to Mr. White for this invention [illegible]

per

per end of the tube *a* should be made conical, and its edges sharp, the better to suffer the papers to pass over it. A section of the tube and female screw *b* beneath is shown separately at fig. 4. The papers are shown in fig. 2, in the situation they are commonly placed upon the cylinder, with the wire within the cylinder.

IV.

On Respiration. By WILLIAM ALLEN, *Esq. F.R.S., and* WILLIAM HASLEDINE PEPYS, *Esq., F.R.S.*

(Concluded from p. 301.)

First Experiment with Atmospheric Air.

Atmospheric air respired by a Guinea pig.

THE Guinea pig was placed upon the stand, and the apparatus arranged as represented in the plate: 250 cubic inches of atmospheric air were admitted into the mercurial gasometer communicating with B: the gasometer communicating with C was quite empty, the apparatus being tried was found perfectly air tight, and the whole quantity of air 310 cubic inches.

The cocks H and I being opened, gentle pressure was made upon the glass of gasometer B, so as to cause the air to pass through A, which consequently drove an equal portion through the tube C into the empty gasometer; a quarter of an hour was employed in passing the gas, which measured exactly 250 cubic inches in C, so that there was no alteration of volume; the cocks H and I were now closed, and the respired air being examined by the usual methods, 100 parts were found to contain

Results.

5	carbonic acid
16	oxigen
79	azote
100	

As the air after the experiment had experienced no alteration of volume, and then contained the same proportion of azote as atmospheric air, this substance had remained unaltered. But

But 15·50 cubic inches of oxigen had been converted into carbonic acid gas.

100 : 5 : : 310 : 15·50.

Summary of the Experiment.

Summary.

Bar.	Therm.	Atmos. air Inspired.	Gas after experiment	Cub. inches of carb. acid.	Cub in. of carb. acid per minute	Time.
30	43°	310	310	15·5	0·62	25 min.

Experiment II. *Atmospheric Air.*

Exp 2.

The experiment was repeated in exactly the same manner; the animal, except from confinement, appeared much at his ease all the time. The air after the experiment contained in 100 parts

5·5 carbonic acid
15·5 oxigen
79 azote

100

Here the proportions of azote were undisturbed, and 17·05 cubic inches of carbonic acid procured.

100 : 5·5 : : 310 : 17·05

Summary of the Experiment.

Summary.

Bar.	Therm	Atmos air Inspired	Air after Experiment.	Carb. acid found.	Carb. acid per minute	Time
29·66	38°.	310.	310	17·05	0·68	25 min.

Experiment III. *Atmospheric Air.*

Exp 3.

The apparatus being arranged as before, we kept the pig in the glass A for one hour, and during that time passed 1060 cubic inches of atmospheric air through it, which measured 1061 : portions of the respired gas had been preserved in the mercurial bath, and the usual trials made upon the mixture, which was found to contain 5 parts of carbonic acid in every 100, or 53 cubic inches in the whole [illegible] the azote was unaltered; 100 : 5 : : 1060 : [illegible]

Summary of the Experiment.

Summary.

Barom.	Therm.	Atmos. air before expt.	Air after expt.	Inc[illegible]	Carb. [illegible]cid	[illegible]
29·8	56°	1060.	1061	1		

Experiment

Experiment IV. Oxigen Gas.

The pig hitherto employed was put into the glass vessel A, which with the tube contained 60 cubic inches of atmospheric air; 250 cubic inches of oxigen, containing 5 per cent of azote, were admitted into the gasometer communicating with B, and during a quarter of an hour were made to pass slowly through the vessel, in which the animal was confined, to the empty gasometer communicating with C, where it measured exactly 250 cubic inches; a portion was preserved in the mercurial bath for examination, and the gasometer B was replenished with 250 cubic inches of the same oxigen; this was passed, in about the same time as before, through A into gasometer C, when it measured 248 cubic inches.

Exp. 4. with oxigen gas.

250 cubic inches more of the oxigen were now admitted into gasometer B, and passed in the same manner through A into C, where they measured 249.

The gasometer B was for the fourth and last time supplied with 250 cubic inches more of the oxigen, which were passed as before, through A into C, during about a quarter of an hour, and then measured 249.

The pig had remained in the vessel one hour and twelve minutes; it did not appear to have suffered in the least; portions of the respired gas were saved from each of the gasometers, and examined as usual.

Results.

	Cubic Inches.		Contained in 100 parts.	Carb. Acid.	Oxigen.	Azote.
No. 1.	250	Carb. acid	8	. 20		
		Oxigen	66		165	
		Azote	26			65
			100			
No. 2.	248	Carb. acid	10	24·80		
		Oxigen	78		193·44	
		Azote	12			29·76
			100			

No. 3.

	Cubic inches.	Contained in 100 parts.		Carb. Acid.	Oxigen.	Azote.
No. 3.	249	Carb. acid	10	24·90		
		Oxigen	80		199·20	
		Azote	10			24·90
			100			
No. 4.	249	Carb. acid	12	29·88		
		Oxigen	79		196·71	
		Azote	9			22·41
			100			
In Glass A, and tube C.	60	Carb. acid	12	7·20		
		Oxigen	79		47·40	
		Azote	9			5·45
	1056		100	106·78	801·75	147·62

Total, gas before experiment,	1060
after	1056
Deficiency,	4

Calculation for Oxigen.

Calculation for oxigen

We do not calculate upon the tube from gasometer B, because it is always in the same state after the experiment as before.

1000 cubic inches of oxigen containing 5 per cent azote, consisted of		950 pure oxigen
60 atmospheric air with the pig, and in tube C, containing 21 per cent oxigen		12·60
Total, oxigen before experiment,		962·60
Oxigen found after experiment,	801·75	
Ditto in carbonic acid	106·78	
		908·53
Oxigen missing,		54·07

Calculation

Calculation for Azote.

Calculation for nitrogen

1000 cubic inches containing 5 per cent azote - - -	50
60 atmospheric air, containing 79 per cent	47 40
Total azote before experiment,	97·40
Ditto found after experiment,	147·52
Increase of azote,	50·12

This increase of azote was much more than equal to the cubic contents of the animal's body, the deficiency of 4 cubic inches was doubtless oxigen absorbed.

Summary of the Experiment.

Summary

Bar	Therm	Oxigen, &c inspired	Gas after experiment.	Deficiency.
29 05	57°	1060	1056	4
Carb. acid found	**Carb acid per min**	**Time.**	**Oxigen missing**	**Azote added.**
106	1·48	1h. 12m.	54·07	50·12

Experiment V. Oxigen.

Exp 5

In this experiment we employed a smaller pig, which occupied the space of 33 cubic inches, and our object was to keep him for the same length of time in a smaller quantity of gas, we therefore only used 750 cubic inches of oxigen, beside the common air contained in the glass A and tube, amounting to 66 cubic inches. The first 250 cubic inches were passed through the glass A into C in 21 minutes, where it appeared to have undergone no alteration of volume; 250 cubic inches more were passed during the next 23 minutes, and these measured 248 in C, the last 250 were passed in 24 minutes, and the volume remained unaltered. The animal did not appear to suffer the slightest inconvenience, except from the confinement.

State of the Gas before Respiration.

State of the gas before respired

		Oxig-n	Azote
66 cubic inches of atmospheric air,	=	13 86	52·14
750 oxigen, containing 5 per cent azote,	=	712·50	37 50
816 [illegible] total consisting of		726 36	89·64

The.

The oxigen was tried before, as well as after the experiment, and both the results agreed perfectly with each other. We now examined portions of gas preserved from the three gasometers, with lime water, and the tests for oxigen.

State of it after.

No.		Time min.	Contained in 100 parts.		Carbonic acid.	Oxigen.	Azote.
No. 1.	250.	24	Carb. acid,	9·5	23·75		
			Oxigen,	60·5		151·25	
			Azote,	30			75
				100			
No. 2.	218.	[illegible]	Carb. acid,	9·5	23·56		
			Oxigen,	81		200·88	
			Azote,	9·5			23·56
				100			
No. 3.	250.	24	Carb. acid,	10	25		
			Oxigen,	82		205	
			Azote,	8			20
				100			
	06 with pig, as No. 3.				6·60	54·12	5·28
	811	71			78·91	611·25	123·84

Calculation for Oxigen.

Calculation for oxigen.

Oxigen before the experiment -		726·36
Ditto after - - -	611·25	
In carbonic acid - - -	78·91	
		690·16
Loss of oxigen		36·20

Calculation for Azote.

Calculation for nitrogen.

Azote found after experiment	[illegible]
Ditto before experiment	[illegible]
Increase of azote	[illegible]

Sup[illegible]

Summary of the Experiment.

Summary.

Therm.	Gas before Exper.	Gas after Exper.	Deficiency.	Carbonic Acid found.
56°	816	814	2	78·91

Cubic Inches per Minute.	Time. h ″	Oxigen missing.	Azot added.
1·11	1 11	36·20	34·20

The quantity of azote added, of oxigen missing, and of carbonic acid formed, were smaller than in the last experiment; but the animal in this instance was smaller, as well as the quantity of oxigen passed through in a given time.

Increase of nitrogen chiefly at the beginning.

In this case, as in the human subject, the increase of azote takes place principally in the early periods. The whole azote contained in the 66 cubic inches, confined with the pig, was only 52·14; but supposing, which perhaps was not the case, that the 66 of common air were expelled by the first 250 cubic inches of oxigen, we should have

$$\begin{array}{rl} & 250 \\ \text{less} & 66 \\ \hline & 184 \text{ of oxigen,} \end{array}$$

containing 5 per cent azote, or 9·20 cubic inches; these added to the 52·14, would make 61·34 of azote to be found in the first gasometer, of respired gas, but we detected 75, so that even on this supposition 13·66 of azote were added in the first twenty-four minutes.

The azote contained in the second gasometer before respiration was 12·50 cubic inches, but after it had been respired for twenty-three minutes, we found 23·75, or an increase of 11·25 azote. The azote contained in the third gasometer, before respiration, was, as before, 12·50 cubic inches; but after it had been respired for twenty-four minutes, we found 20, or an increase of 7·50 azote.

The azote contained in the 66 cubic inches was 3·30, but we found 5·[illegible], or an increase of 1·98 azote.

Blood disposed to give out nitrogen, when in deficient proportion

From the results of these experiments it seemed, that [illegible] the *usual proportion* of azote was not present in the [illegible]pired, there was a disposition in the blood to give out a [illegible] quantity [illegible] exchange for an equal volume of oxigen, and we resolved to try, whether this circumstance

would

would occur when hidrogen was substituted for azote. We accordingly made a mixture containing 22 per cent oxigen, and 78 hidrogen.

Experiment 6. Hidrogen and Oxigen.

Experiment 6. Oxigen mixed with hidrogen.

The pig employed in the last experiment was placed upon the stand in the glass A, with 66 cubic inches of common air as usual.

250 cubic inches of the mixture were passed from the gasometer communicating with B, through the glass A, into the gasometer communicating with C, during sixteen minutes. The animal did not appear uneasy: a second quantity of 250 cubic inches was passed in seventeen minutes and three quarters: the animal did not seem to be in the least incommoded.

A third quantity of 250 cubic inches was passed, in about sixteen minutes.

A fourth quantity of 250 cubic inches in eleven minutes and three quarters; but during this time, the animal became very sleepy, and towards the end of the experiment, kept his eyes constantly shut; he, however, appeared to suffer nothing, and was easily roused for a short time by rapping at the side of the glass. At the end of sixty one minutes and a half, he was taken out, and we found, that during this time he had produced 60·20 cubic inches of carbonic acid gas, or rather less than one cubic inch in a minute.

Less carbonic acid evolved than with oxigen.

It appears, that less carbonic acid was evolved in this instance in a given time, than when oxigen was respired, but some circumstances occurred to prevent us from discovering what change the azote had experienced: this point was, however, decided by the following experiment.

Experiment 7. Hidrogen and Oxigen.

Experiment 7. Oxigen with hidrogen.

Having mixed hidrogen and oxigen gases in such proportion, as that the oxigen should rather exceed the quantity contained in atmospheric air, we placed the same animal in the glass A with 66 cubic inches of atmospheric air. 250 cubic inches of the mixture were admitted into gasometer B, from the large water gasometer, and gradually passed through the glass A into gasometer C, during fifteen minutes. The pig

pig did not appear uneasy, and the respired gas measured 250 in C: a portion of this was preserved for examination, which we shall call No. 1.

250 cubic inches more of the mixture were admitted into B, and gradually passed, as before, during thirteen minutes; it measured 250 in C; and a portion No. 2 was preserved for examination.

The animal did not seem to suffer any inconvenience. 250 cubic inches more of the mixture were admitted into B, and gradually passed, as before, through A into C during seventeen minutes. The animal now became quite sleepy, but did not appear to suffer any thing. He was taken out at the end of forty minutes.

Residue of the mixture examined.

At the close of the experiment, the remains of the mixture, which had stood about an hour in the large water gasometer, being examined, were found to contain 22 per cent of oxigen, and no carbonic acid; of the residual 78 parts, 20 were mixed with 10 of oxigen, which had been previously found to contain 3 per cent azote; these 30 parts being detonated in Davy's improved Volta's eudiometer, by the electric spark, were reduced to 3 parts; and these 3 parts, being treated with the tests for oxigen, were reduced to 2 parts, *a proof that all the hidrogen had been consumed;* but the 10 parts of oxigen contained 3 of azote; these deducted from 20 leave 1.7 for the azote contained in 20 parts of the residuum 78.

20 : 1.7 :: 78 : 6·6

The mixture employed, therefore, contained in every 100 parts

22	oxigen
6·6	azote
71·4	hidrogen
100	

We next examined the gas which had been respired.

Gas 1. examined.

No. 1, 250 cubic inches respired during fifteen minutes. 100 parts subjected to the action of lime water in Pepys' Eudiometer, were reduced to 93·5; and these by the tests for oxigen were farther diminished to 77. 20 parts of this 77 being mixed with 10 of oxigen and detonated, the residuum,

duum, treated with the tests for oxigen, left 12 parts, which were azote.

From these 12 parts
Deduct 0·3 for the azote in the 10 parts oxigen

Leaves 11·7 for the azote contained in 20 parts of the residual 77.

20 : 11·7 :: 77 : 45

Its component parts.

No. 1 therefore consisted in 100 parts of

6·5	carbonic acid
16·5	oxigen
45	azote
32	hidrogen
100	

Gas 2 examined.

No. 2. 250, respired during thirteen minutes; 100 parts were reduced by lime water to 92·5, and these by the tests for oxigen to 77. Of these 77 parts, 20 being mixed with 10 of oxigen, and detonated, were diminished to 4; and these 4 being examined for oxigen left 3, which must be azote:

From these 3
Deduct 0·3 for azote in the 10 parts oxigen,

Leaves ·2 7 for the azote contained in 20 parts of the residual 77.

29 : 2·7 :: 77 : 10·4

Its component parts.

No. 2 therefore consisted in 100 parts, of

7·5	carbonic acid,
15·5	oxigen,
10·4	azote,
66·6	hidrogen,
100	

Gas 3. Its component parts.

No. 3. 250, respired during seventeen minutes; examined as above, consisted in 100 parts, of

6	carbonic acid,
17	oxigen,
6·5	azote,
70·5	hidrogen,
100	

The

The 66 remaining with the animal at the close of the experiment may be considered as very nearly the same as No. 3.

Less carbonic acid evolved during sleep.

In this, as in the former experiment, we observed, that the evolution of carbonic acid was greatest at the middle of the time, but was considerably diminished toward the end, as the pig became sleepy; it is not improbable therefore, that during sleep, less carbonic acid is evolved than when the animal is exercising all its faculties.

More carbonic acid evolved than oxigen consumed.

When atmospheric air alone is respired, we have uniformly found, that the carbonic acid evolved, added to the oxigen remaining, exactly equalled the oxigen existing in the air before it was respired; but in the present instance it was one per cent more, a circumstance which we are at present unable to account for, but it was constantly the case in all the three trials.

Calculation for Azote.

Calculation for nitrogen.

From the foregoing statement we are enabled to ascertain the quantities of azote, both before and after the experiment.

Azote before the Experiment.

66 cub. inches atmospheric air, with the animal, tained $\frac{79}{100}$ or - - -	52·14
750 ———— of the mixed gasses contained $\frac{66}{1000}$ or	49·50
816 total gas employed	101.64

The total azote before the experiment was therefore 101·64 cubic inches.

Azote after the Experiment.

		Respired during		
No. 1.	250.	15 min.	100 : 45 :: 250 :	112·50
2.	250.	13 min.	100 : 10·4 :: 250 :	26
3.	250.	17 min.	100 : 6·5 :: 250 :	16·25
4.	66·		100 : 6·5 :: 66 :	4·29
	816	45 min.	Azote after experiment	159·04
			Ditto before	101·64
			Increase of azote	57·40

Calculation

Calculation for Hidrogen.

Calculation for hidrogen.

Hidrogen before Experiment.

The mixture before the experiment was found to contain 71·4 hidrogen.

100 : 71·4 :: 750 : 535·50

therefore the total quantity must be 535·50 cubic inches.

Hidrogen after Experiment.

No. 1.	250	100	:	32	::	250	:	80
2.	250	100	:	66·6	::	250	:	166·50
3.	250	100	:	70·5	::	250	:	176·25
	66 in A	100	:	70·5	::	66	:	46·53

Hidrogen found after experiment	469·28
Hidrogen before the experiment	535·50
Ditto after - -	469·28
Loss of hidrogen - -	66·22

More nitrogen evolved in the early than in the later periods

In this experiment, as well as in those with oxigen, the proportion of azote evolved was greater in the early than in the later periods, and it becomes interesting to contrast them: thus we know that 52·14 cubic inches of azote were in the vessel with the animal at the beginning of the experiment, and that, of the 250 cubic inches of mixed gases passed in the first fifteen minutes, only 184 could be expelled into gasometer C, (100 : 6·6 :. 184 : 12·14.) which contained only 12·14,

making together 64·28 of azote, which was all that could have been expected in the first gasometer of 250 after respiration, supposing the *whole* of the common air had been expelled, but we detected 112·50, or an increase of 48.22 cubic inches in fifteen minutes.

The second gasometer, before it was connected with the glass A, contained 16·50 cubic inches of azote; we found however about 26, and what is remarkable, in the last gasometer there was no increase at all.

Calculation

Calculation for Carbonic Acid.

Calculation for carbonic acid.

No. 1.	250. 15 min.	100 : 6·5 :: 250 :	16·25
2.	250. 13 min.	100 : 7·5 :: 250 :	18·75
3.	250. 17 min.	100 : 6 :: 250 :	15
	66.	100 : 6. :: 66 :	3·96
	45.		53·96

The quantity of carbonic acid evolved in 45 minutes was, therefore 53·96 cubic inches, or at the rate of 1·19 cubic inches per minute.

The foregoing experiments seem to prove,

General conclusions.

1. That when atmospheric air alone is respired, even by an animal subsisting wholly upon vegetables, no other change takes place in it, than the substitution of a certain portion of carbonic acid gas for an equal volume of oxigen.

2. That when nearly pure oxigen gas is respired, a portion of it is missing at the end of the experiment, and its place supplied by a corresponding quantity of azote; the portion evolved in a given time being greater in the early than in the later periods.

3. That the same thing takes place when an animal is made to breathe a mixture of hidrogen and oxigen, in which the former is in nearly the same proportion to the latter, as azote to oxigen in atmospheric air.

4. That an animal is capable of breathing a mixture of 78 parts hidrogen and 22 oxigen for more than an hour, without suffering any apparent inconvenience.

5. That the excitability of an animal is much diminished when he breaths any considerable proportion of hidrogen gas, or that it at least has a tendency to produce sleep.

6. That there is reason to presume an animal evolves less carbonic acid gas during its sleeping, than in its waking hours.

7. That the lungs of a middle sized man contain more than 100 cubic inches of air after death.

The facts not explicable at present.

These experiments have been conducted without reference to any particular theory, and indeed some of the results were so contrary to our preconceived opinions, that we have been induced to bestow more than ordinary attention on the subject. Confident, however, that all those who repeat

repeat the experiments with the same care will arrive at the same results, we shall rest satisfied with stating the facts, not without a hope, that those brilliant discoveries of Professor Davy, which have already given us new views of the operations of nature, will in their progress furnish us with that explanation, which it is in vain to expect at present.

Nitrogen.

Azote, or nitrogen, for instance, has been considered as a simple or elementary substance; it is recognised, however, principally by negative properties. Every gaseous fluid, which will not support life or combustion, which is not absorbed by water, or acted upon by the tests for oxigen, or capable of being detonated with oxigen gas, is generally pronounced to be azote: it is the constant residuum in almost all our experiments upon gases, but who shall say whether this residuum is a simple substance or a compound?

Is it a simpl or a compound?

Is it the oxide of a metal?

The experiment of Professor Berzelius leads us to suspect it of metallic properties; and those of Davy make it probable, that it is an oxidated body; the subject is still under discussion. But we may fairly indulge more than a hope, that the ardent zeal, and well directed labours of the philosophers just mentioned, will throw a new and important light upon this obscure and difficult subject.

V.

Memoir on the Prussic Acid. By Mr. R. Porrett, *Junr., of the Tower* *.

Difference of opinion respecting the prussic acid.

CONSIDERABLE differences of opinion exist among the most celebrated chemists respecting the composition of the prussic acid, some agreeing with Fourcroy and Vauquelin, that oxigen is one of its component parts; and others with Berthollet and Proust, who dispute its presence. Mr. Proust, in his history of the Prussiates, asserts, "That there is no fact, that indicates oxigen to make a part of this

* Trans. of the Society of Arts. The silver medal was voted to Mr. Porrett for this communication.

this acid; and that from the well-known affinities of its three elements, added to the circumstances under which it is formed, it can scarcely be thought that it does." This difference of opinion implies a want of some decisive experiments, which may set the question for ever at rest, and those which I am going to relate I am induced to think are of that description.

Some time back, I proposed to myself the discovery of a method of preparing a triple prussiate of potash, in a pure state, which should be free from the objections to which the processes in general use are subject. In reflecting on the means most likely to attain this end, it occurred to me, that I should succeed, if I decomposed prussiate of iron by double elective attraction rather than by single; employing, instead of a pure potash, this alkali in combination with a substance uniting the properties of solubility when combined with potash, strong attraction for oxide of iron, and insolubility when united to this oxide. The only substances I could think of possessing all these requisite properties were the succinic acid and sulphur. As the high price of the former precluded its use for this purpose, I determined to employ the latter. I therefore took one ounce of dry sulphuret of potash, and one ounce and a half of the best prussian blue, previously well washed and powdered, and put them into a Florence flask, two thirds filled with distilled water; a disengagement of sulphuretted hidrogen of ammonia, and of caloric, immediately took place. The materials were boiled slowly together for three hours, occasionally replacing the water which evaporated. The whole was then thrown on a filter; what remained on the filter was black, and consisted of sulphuret of iron, and undecomposed prussiate of iron. The liquid, that passed through, I found on trial to consist of triple prussiate of potash, and hidroguretted sulphuret of potash. In order to complete the decomposition of the latter, I boiled the liquid again, for the same time as before, with another half ounce of prussian blue, and when cold filtered it. The filtered liquid (A) was now nearly colourless, and free from hidroguretted sulphuret. On pouring a little of it into a solution of oxisulphate of iron, I was very much surprised to find that

Attempt to discover an improved method of preparing triple prussiate of potash.

Sulphuret of potash boiled with prussian blue.

The solution boiled with fresh prussian blue

turned red with oxisulphate of iron, without precipitation.

that solution changed to a deep blood-red colour, without any precipitate ensuing, instead of forming with it a precipitate of blue prussiate of iron. So unexpected a phenomenon determined me to undertake an examination of this liquid; with this view I subjected it to the action of the chemical agents mentioned in the following table.

TABLE I. with Liquid A.

Action of different tests on this solution.

CHEMICAL AGENTS.	EFFECTS.
Paper stained with turmeric	No change of colour.
Paper stained with litmus	Do.
Potash - -	No disengagement of ammonia, or any apparent change.
Lime - - -	Do.
Diluted sulphuric acid	An expulsion of sulphurous acid, the liquid becomes slightly opalescent.
Nitric acid (pure)	The acid assumes a red colour, but this effect is not permanent.
Oximuriatic acid -	This acid loses its smell.
Muriatic acid (pure)	No change.
Muriate of barytes	A white precipitate.
Tincture of galls -	No change.
Nitro-muriate of platina	A heavy, brilliant, ochre-yellow precipitate.
Muriate of gold -	Dark olive brown precipitate.
Nitrate of silver -	A precipitate at first white, but quickly passing to yellow, red, and lastly to brown.
Sulphate of silver -	A dull white or stone coloured precipitate.
Oxinitrate of mercury	A white precipitate.
Oxinitrate of lead -	A white precipitate.
Supersulphate of copper	A dull white precipitate.
Muriate of bismuth	No precipitate.
Sulphate of iron -	No change.
Oxisulphate of iron -	The solution assumes a deep blood-red colour. No precipitate.

The liquid contained sulphite of potash and some other principle.

The effects of the sulphuric acid and of the muriate of barytes clearly proved the existence of sulphite of potash in the liquid; while that of the oxisulphate of iron indicated the presence of some other principle, to which the liquid was indebted for its peculiar characters. The separation of this principle in a pure state became therefore a necessary preliminary operation to its examination; after a few trials, I succeeded in effecting this separation. The following is the process I employed.

The

The liquid was evaporated by a gentle heat to dryness; upon the saline residuum alcohol was poured, till it ceased to extract any thing; by this means the whole of the sulphite and sulphate of potash was left behind, and the alcohol when filtered held in solution that part only, which had the red tingeing property with solutions of iron. The alcohol was now got rid of by distillation, and the salt it left in the retort was redissolved in water. This solution (B) gave the following results with the different metallic solutions.

This dissolved in alcohol, evaporated to dryness, and redissolved in water.

TABLE II. with Liquid B.

Action of metallic salts on this solution.

METALLIC SOLUTIONS.	EFFECTS.
Nitromuriate of platina	A precipitate similar to that in Table I, but in a smaller quantity, and longer in forming.
Muriate of gold	Light olive precipitate, some gold reduced.
Nitrate of silver	A grayish white precipitate, not changing colour.
Sulphate of silver	A clear white precipitate.
Nitrate of mercury	A copious white precipitate.
Oxinitrate of mercury	A white precipitate in small quantity.
Nitrate of lead	No precipitate.
Oxinitrate of lead	No precipitate.
Superacetate of lead	No precipitate.
Hyperoximuriate of lead	A slight white precipitate.
Supersulphate of copper	A dull white precipitate.
Muriate of tin	No precipitate.
Muriate of bismuth	No precipitate.
Sulphate of iron	No change.
Oxisulphate of iron	Same as Table I.
Oxisulphate of manganese	The crimson colour disappears; no precipitate.
Sulphate of Zinc	No change.
Nitromuriate of cobalt	No precipitate.
Nitrate of nickel	No change.

It is necessary to remark, that in the preceding Table, as well as in Table I, several of the nitrates and muriates were slightly reddened, though not in a degree to be compared with the oxisulphate of iron. I have not noticed this in the table, because I am not certain, whether this effect was not owing to a minute portion of oxide of iron, which might have been introduced into those solutions by the acids employed to make them, as both the nitric and muriatic acids of commerce generally contain some; an excess of nitric acid,

The solution a little reddened by some of the nitrates and muriates,

acid, even if pure, might also cause this effect, as Table I may convince us. The solutions with which this effect occurred to me were those of bismuth, silver, mercury, lead, cobalt, gold, and platina.

not affected by the air. The liquid B is not altered by exposure to the air.

State of the sulphate unimportant. Its effect on oxisulphate of iron is the same, whether this sulphate is neutral, or contains an excess of acid, or is supersaturated with carbonate of ammonia.

Sulphuric acid destroys the colour, unless much water present. Sulphuric acid destroys the colour produced on oxisulphate of iron, provided the three liquids are in a concentrated state. If there is much water present, no change ensues.

How is the principle formed? Having obtained the tingeing principle B separate from the other salts with which it was contaminated, I asked myself, to what was its formation and the simultaneous disappearance of the prussic acid, during the second ebullition, owing? I could imagine but five causes for this that were likely to have been efficient, concerning each of which I made a question to be resolved by experiment, viz.

Question 1. Was it owing to the complete separation of the oxide of iron from the triple prussiate by the sulphur, and the subsequent decomposition of the simple prussiate by the heat of ebullition long continued?

Question 2. Was it owing to the action of the sulphurous acid produced?

Question 3. Was it owing to the action of the sulphuretted hidrogen?

Question 4. Was it owing to a combination of the prussiate of potash and sulphur?

Question 5. Was it owing to the deoxidation of the prussic acid by the hidrogurretted sulphuret?

Question 1 answered. To answer the first question, it is only necessary to attend to the results afforded by long-continued boiling of the simple prussiate of potash. I shall state these results as I find them recorded by Professor Proust.

Results of long boiling the simple prussiate. They are carbonate of ammonia, carbonate of potash, and some simple prussiate that escapes decomposition, even after four or five successive distillations. There is, therefore, no analogy between the products of this experiment and the liquid A; for had the latter contained carbonate of potash, it must have changed turmeric paper brown; had it contained

ed carbonate of ammonia, it must have done the same, and likewise have given out ammoniacal gas when potash and lime were added; it must also have turned blue the solution of copper; and had it contained prussiate of potash, it must have produced prussiate of iron, when added to the green sulphate of this metal: but it will be seen by referring to Table I, that none of these effects were produced. Were farther evidence necessary of the dissimilarity of the two liquids, it might be mentioned, that Professor Proust poured alcohol on the saline residuum of his distillation of the prussiate, which took up a part that he found to be prussiate of potash; had any of the tingeing salt B been present, the alcohol must have dissolved this likewise, and it could not have escaped his observation. We have therefore ample grounds for negativing the first question.

Question 2 answered.

In order to answer the second question, I passed sulphurous acid gas for a long time through a solution of triple prussiate of potash; the prussic acid was expelled, and sulphite of potash formed; but this sulphite was not mixed with any tingeing salt. On the supposition, that the disappearance of the prussic acid, in the liquid A, might have been owing to its having been expelled entirely by the sulphurous acid; and that the tingeing liquid resulted from the mutual action of the other principles, namely, the oxide of iron and hidroguretted sulphuret of potash; I subjected a mixture of these materials to long boiling, but could not by this means produce a liquid, that tinted oxisulphate of iron red. Sulphurous acid gas, passed through water in which Prussian blue was diffused, did not in the least affect this compound. These experiments completely refute the opinion, on which the second question was grounded.

Question 3 answered.

To enable me to reply to the third question, I passed sulphuretted hidrogen gas for several hours through a solution of triple prussiate of potash, on which it was found to have no effect.

Question 4 answered.

We shall be little disposed to allow, that there is any foundation for the fourth question, when we consider the circumstances of the last-mentioned experiment, in which sulphur in the state of the most minute division was offered to the triple prussiate, without any combination ensuing;

 and

and also when we compare the effects of the metallic solutions in Table II with those which would ensue with liquids containing sulphur. But, if any doubt should still be entertained on this subject, the following experiment will perhaps remove it. Into a solution of prussiate of mercury throw some pieces of phosphuret of lime; the oxide of mercury of this prussiate will thus be reduced, and separated from the liquid, which is to be filtered. Some of this liquid poured into carbonate of iron turns it red, the red colour soon disappears, and a white precipitate begins to form; this white precipitate soon changes to green, and, if a little nitric or oximuriatic acid be now poured upon it, it becomes a perfect blue prussiate of iron. This experiment, in which a liquid turning a solution of iron red was produced without the employment of a particle of sulphur, goes very far to negative our fourth question; and when considered in conjunction with the preceding ones, we can hardly do otherwise than dissent from the supposition, which gave rise to that question.

Question 5 answered.

But if the experiment last adduced tends to refute the fourth question, it very strongly supports the fifth; for the changes of colour observable were undoubtedly owing to successive stages of oxidation by the contact of the atmosphere. In confirmation of this question, it may likewise be asserted, that the long boiling with the hidroguretted sulphuret is a powerful deoxidating process. But it will be said to me, if it is really true, that the prussic acid has been deoxidated by this process, you ought to be able to recompose that acid from the solution B by oxidation. This struck me very forcibly; and being anxious to give this last proof of the truth of my deductions, I attempted the recomposition of this acid by several oxidating processes for some time without success. I had at last, however, the particular satisfaction of succeeding completely by the agency of nascent hyperoximuriatic acid. The method I employed was the following.

The prussic acid is deoxidated,

and may be recomposed by oxidation

Process.

A little hyperoximuriate of potash was put into the bottom of a glass tube. Over this some of the liquid B, mixed with a few drops of diluted sulphuric acid, was poured. The heat of a candle was then applied to the bottom of the tube,

tube, and as soon as a violent action commenced, the heat was withdrawn. By this process the prussic acid was reproduced, as was proved beyond the possibility of a doubt by the formation of blue prussiate of iron, when poured into a mixture of green and red sulphate of this metal. Blue prussiate may also be produced at once, by substituting for the diluted sulphuric acid a solution of green sulphate of iron with excess of acid.

Necessary precaution.

It is essential to the success of this experiment, in which the prussic acid is regenerated from the liquid B by the nascent hyperoxigenised muriatic acid, that the excess of acid remaining in the liquid, after the oxigenising process, should be neutralized by an alcali previous to pouring it into the solution of iron, which should likewise be perfectly neutral.

The new principle hitherto combined with potash.

Having thus succeeded in proving, that the tingeing principle of the liquid B was suboxidised prussic acid, my next object was to obtain this principle in a free state; for we must recollect, that we have hitherto considered it only in combination with potash, with which it formed a neutral salt. This circumstance gave me reason for supposing it an acid, and I therefore determined to attempt its separation by abstracting its base by a stronger acid. The following was the process I employed for the purpose.

Obtained as a separate fluid.

The liquid B was evaporated nearly to dryness, and put into a retort with diluted sulphuric acid; a receiver was then adapted to it, and about two thirds of the liquid distilled over by a gentle heat; what remained in the retort was sulphate of potash. The receiver contained a colourless liquid, with a faint, sour, disagreeable smell, and a decided acid taste. This liquor I have named, in conformity with the principles of the new nomenclature, *prussous acid*, and its salts *prussites*, of which the liquid B contained one in solution, namely the prussite of potash.

The prussous acid.

The effects of the prussous acid on the earthy and metallic solutions, as far as I have tried them, are noted in the following table.

TABLE

TABLE III. with Prussous Acid.

Action of the prussous acid on earthy and metallic solutions

CHEMICAL AGENTS.	EFFECTS.
Muriate of lime	No change.
Muriate of barytes	No change.
Muriate of gold	The gold precipitated metallic.
Sulphate of silver	Copious white precipitates.
Nitrate of silver	Copious white precipitates.
Prussiate of mercury	No change.
Nitrate of mercury	Copious grayish white precipitate
Oxinitrate of mercury	Very slight precipitate, white.
Oxisulphate of iron	Solution turns blood-red, no precipitate.
Nitromuriate of platina	No precipitate.
Nitrate of lead	No change.
Oxinitrate of lead	Solution becomes red, but hardly any precipitate formed, unless heated, in which case a copious white precipitate ensues. The red colour disappears, a rapid action takes place between the two liquids, and some of the nitric acid of the solution is decomposed.
Hyperoximuriate of lead	A slight precipitate, probably of muriate of lead.
Supersulphate of copper	Solution becomes slightly turbid.
Muriate of bismuth	No precipitates.
Nitrate of nickel	No precipitates.
Muriate of tin	No precipitates.
Nitrate of cobalt	No precipitates.
Sulphate of iron	No precipitates.
Sulphate of manganese	No precipitates.
Sulphate of zinc	No precipitates.

Readier process for preparing prussiate of potash.

I cannot conclude this part of my memoir without giving a more simple and expeditious process for preparing prussite of potash, than that which I at first discovered. It is the following.

Pour a solution of prussiate of mercury into hidrogu-retted sulphuret of potash, till the mutual decomposition of the two liquids is completed; prussiate of potash is instantly formed, and may be separated by filtration from the solid combination of the sulphur and mercury.

Properties in the first process.

I wish also to observe, that the proportion of Prussian blue I have mentioned for boiling with the sulphuret is much larger than is necessary, as I have since succeeded in obtaining prussiate of potash when the proportion of Prussian blue was only equal to that of the sulphuret, but long boiled

boiled with the latter in two distinct and equal portions. The prussiate of potash thus obtained is, however, mixed with a much larger quantity of hidroguretted sulphuret than when a greater portion of Prussian blue is employed.

Use of this acid

Whether the prussous acid can be applied to any use, time and future experiments must decide. It appears to me to be a very delicate test of silver and of iron in solution.

The prussic acid contains more oxigen,

The preceding experiments, by proving the presence of oxigen in prussic acid, give it a stronger claim than it before possessed for being placed among the acids.

but is less acid than the prussous

The prussous acid possessing stronger acid properties than the prussic is a curious, though not a solitary, instance of the effect of oxigen in diminishing acidity, when its quantity exceeds a certain fixed proportion; in this respect the prussic acid is analogous to the oximuriatic.

Pure triple prussiate produced

To recur to the attempt which gave rise to the researches that are the subject of this memoir, I beg leave to state, that I have succeeded in producing pure triple prussiate of potash, by stopping the process before the change which produced the prussite ensued; and by subsequent purification of the lixivium from sulphates and sulphites, by acetate of barytes; from sulphur by acetate of lead; and, lastly, from the acetate of potash thus formed by crystallization; but on account of the complication of this process, I hesitate to recommend it for general use.

ROBERT PORRETT, Jun.

Tower, London, April 21, 1809.

VI.

On the Oxides of Iron, and the Manner in which they comport themselves with some Acids. By Mr. Bucholz *.

Proportion of oxigen in iron oxides not settled.

THE little agreement among chemists with respect to the proportions of oxigen in oxides of iron induced Mr. Bucholz to make these experiments. He employed for the purpose bright iron wire of the greatest purity.

* Annales de Chimie, vol. LXV, p. 202. Abridged from Gehlen's Journal by Mr. Vogel.

 A. *Experiments*

A. *Experiments to determine the Quantity of Oxigen in Red Oxide of Iron.*

Red oxide

1. On a hundred grains of the iron abovementioned, in the state of filings, an ounce of water was poured; and nitric acid at 1·265 was added gradually, till the iron was entirely dissolved.

contains 0 295 of oxigen.

The liquid being cautiously evaporated, and the matter having remained at a red heat for a quarter of an hour, its weight was increased forty two grains. This result differs from those of Lavoisier and Proust.

2. The same experiment repeated with this difference, that sometimes half an ounce of nitric acid, at others an ounce and half were employed, the results were the same.

B. *Experiments on the Black Oxide of Iron.*

Black oxide.

Mr. Bucholz made many attempts to obtain the black oxide. He tried calcination, the action of cold water on iron filings, and the precipitation of green muriate of iron by ammonia and by potash*. Though these alkalis, says Mr. Bucholz, did not produce me pure black oxide, their addition in small quantities to the muriate of iron taught me, that the first precipitate of a greenish white, and the second of a blueish green, consisted of black oxide of iron only retaining more or less muriatic acid; whence it follows, that these precipitates are not to be considered as distinct oxides.

Ammonia and potash precipitate muriate of iron,

not free from acid.

Aqueous vapour converts red hot filings into black oxide

Water in the state of vapour being made to pass through iron filings heated red hot, a black powder was obtained, possessing all the characters of an oxide of iron at a minimum.

A hundred grains of this black oxide were boiled with six ounces of nitric acid at 1·265. The solution was complete. Being evaporated and strongly calcined in a crucible, a red powder remained, weighing 110 grains. The black oxide therefore consists of iron 0·77, oxigen 0·23.

containing 0 23 of oxigen.

* Mr. Bucholz insists much on the smell of phosphorus, or of phosphuretted hidrogen gas, emitted when ferruginous salts are precipitated by ammonia.

Examination

Examination of the Red Oxide of Iron at a high Temperature.

A hundred grains of red oxide were put into a Hessian crucible previously weighed, and this was placed in a larger. The whole was kept at a white heat in a forge fire for three quarters of an hour. The bottom of the lower crucible was vitrified. The oxide was become a blackish steel gray powder, with a metallic lustre, attractable by the magnet. It had lost three grains.

Red oxide exposed to a white heat

The same quantity of red oxide was kept at a white heat for an hour and half. Its loss then was six grains and half. The lower stratum of oxide was completely fused, of a clean fracture, a steel gray, and a metallic lustre. The upper stratum was imperfectly fused, crystallized at the surface, and porous internally. It was a mixture of the red and black oxides of iron.

converted partly into black oxide.

Though a white heat causes the red oxide to pass to the state of black, there is no fear of a red heat producing this effect.

A red heat has not this effect.

Manner in which Iron and its Oxides comport themselves with Nitric Acid.

Nitric acid greatly diluted with water dissolves iron, and produces the nitrate in which iron is oxided at a minimum. Proust and Davy had already shown the same. This state however possesses little permanence, for if the liquid be heated ever so little, or the acid be not very weak, a nitrate at a maximum is obtained.

Action of nitric acid on iron,

The latter being boiled down quickly, set to cool, and cold water poured on it, a red substance insoluble in water, but soluble in nitric acid, was separated. This red substance gave out red fumes when calcined, and a red oxide of iron remained.

The nitrate of iron is not decomposed, when it is boiled down very gently. It leaves a thick residuum of a brown red colour, which attracts moisture from the air, and is perfectly soluble in water.

The red oxide of iron is soluble in nitric acid in a certain degree by means of heat. This action of the nitric acid however

and on the red oxide.

however is much more powerful when it acts on red oxide recently precipitated from a ferruginous salt by means of potash.

Sulphuric Acid and Red Oxide of Iron.

Action of sulphuric acid on red oxide of iron

An ounce of green sulphate of iron dissolved in an ounce of water was made to boil, and six drachms of nitric acid, previously diluted with six drachms of water, were added gradually. A brownish yellow powder fell down, which, after desiccation, weighed 42 grs. [$\frac{7}{12}$ of a drachm, or 35grs. Eng.]. It was the neutral red sulphate of iron with excess of oxide mentioned by Mr. Thenard.

The supernatant liquor, of a reddish brown colour, was heated in a capsule; and immediately a brownish substance, similar to the preceding, separated, which however dissolved in proportion as the liquid evaporated. The thickened matter rapidly attracted moisture from the air, and resumed a sirupy consistence. Sulphuric acid immediately changed its red colour white. Evaporated to dryness with one drachm of sulphuric acid, and heated till all the excess of acid had disappeared, a white powder remained, which exhibited the following properties.

It was tasteless; and insoluble in water, either cold or boiling; but the latter deprived it of a little sulphuric acid, and changed its white colour to a brown.

On agitation with ammonia it changed to a brownish yellow, and the supernatant fluid contained sulphate of ammonia.

Muriatic acid dissolved it entirely by the assistance of heat, and exhibited a yellowish red liquid. Water acidulated with sulphuric acid had the same effect.

Here then we have a sulphate of iron of a white colour, in which the oxide is red and at a maximum of oxidation.

Recapitulation of the Facts in this Paper.

General results

1. There are but two degrees of oxide of iron; the black oxide at a minimum, and the red at a maximum. No other state of oxidation exists in reality, and what has been taken for oxide has been nothing but a combination of one of these oxides with some acid, or a mixture of them in different proportions.

2. A

2. A hundred parts of black oxide contain 77 of iron and 23 of oxigen: The red oxide is composed of 70·5 iron, and 29·5 oxigen.

Oxides of iron.

3. The red oxide of iron, on being exposed to a white heat, is brought back to the state of black oxide: but a red heat does not change its nature, though it may alter its colour.

4. The change of black oxide to red is accelerated by calcination with caustic potash.

5. An oxide may contain one half of oxide at a maximum, notwithstanding it is strongly attracted by the magnet.

6. The black oxide is with difficulty soluble in nitric acid, requiring to be long boiled in concentrated acid.

7. The red oxide is not so insoluble in nitric acid as has been supposed.

8. Lastly, the red oxide may form different combinations with sulphuric acid; one of a reddish brown, and soluble; the other white, and insoluble in water, but soluble in acids.

VII.

Letter from Captain William Bolton *of the Royal Navy, on his improved Jury Masts, and on a Contrivance for better securing the Shrouds of Ships*.*

SIR,

Advantages of captain Bolton's method of rigging jury masts.

FINDING some doubts were entertained relative to the practicability of rigging jury masts, upon the plan I had the honour

* Trans. of Soc. of Arts, vol. xxvii, p. 189. Some doubts having arisen as to the practicability of rigging Captain William Bolton's improved jury masts, described in vol. xxvi, page 167, of the Society's Transactions, (see Journal, vol. xxiv, p. 44) he in consequence has sent to the Society another model thereof completely rigged, which obviates all difficulty therein; and likewise has added another invention of his contrivance, for the better securing the shrouds of rigging; explanatory engravings of both are annexed, and the model is preserved in the Society's Repository. The gold medal of the society was voted to Captain Bolton for his naval improvements.

honour to present to the Society in Oct. 1807, I beg you will lay before them a model completely rigged; from which it will appear, that the rigging so placed will give, from the angle being more obtuse, greater security, and supersede the necessity of cathur pinning [catharpings], and admit of the lower yards bracing sharper up.

Improvement in reefing topmasts.

The hint which I gave formerly relating to striking a topmast is now carried into effect, by the mode in which I have fitted the topmast rigging, which admits of its being set up, almost instantaneously, at any point the mast shall descend to; the advantages of which are many, more particularly when the ships are riding hard in open roadsteads; for, by striking the topmast, ships will be enabled to ride much longer at their anchors, and still be ready to make sail on their topmasts, according to the state of the weather, which will or ought to determine the distance to which the masts are struck.

Improvement in setting up.

In this model I also present to the Society a plan, the earliest of my mechanical pursuits, for setting up a ship's lower rigging, which will be effected by one man; whereas in the ordinary way it is performed by an assemblage of tackles, and the labour of a whole ship's company: the larboard side of the model is fitted up according to the old plan; the starboard side* with my improvement, which consists simply of a screw attached to each shroud, and tightened by a nut under the channels, which should be well secured by iron clamps or knees for this purpose: the expense will be far less than in the common mode; and I do think the bare inspection of the model will be sufficient to establish its superior efficacy.

I will thank you to expunge the passage from page 168 in the last volume, after the full stop at *main-top* †, and substitute the following:

"The spare fore top-mast passes through a cap made from strong plank, *b*, into the square holes of which the heads of the two temporary masts above-mentioned are inserted, and the heel of the topmast is fidded on the tressle trees

* The larboard side in fig. 1 of the plate.

† Journal, vol. xxiv, p. 45, line 13 from bottom.

trees or top as in common, and the mast rigged as usually. The object of the strengthening cap, G, is to steady the spars, and also serves to fid the topmast on, if thought necessary."

I have the honour to be,

Your obedient humble servant,

WM. BOLTON.

Reference to the Drawing of Captain Bolton's Rigged Jury Mast, and his improved Mode of securing the Shrouds, Pl. IX, Fig. 1, 2, 3, and 4.

Explanation of the plate.

AA, fig. 1, represent the partners or pieces of timber, which are bolted to the quarterdeck for the mast to rest upon. B is the stump of the lower mast, which is cut square at the top, and of the same size as the head of the mast originally was; upon this square the main and spare lower caps *a a* are fixed; two mortices must be cut in the partners A A, to receive squares made at the lower ends of the two temporary masts DD, which are supported by the caps *a a*, one of them is a spare main topmast, the other a hand-mast; these two support the main top E, additional squares being made on the tressel trees to receive each of them. *b* is a cap shown in fig. 3, made of four-inch plank doubled for the purpose, and fitted upon the heads of the masts DD, for a fore topmast F F, the heel of which, when struck, rests in a mortice made in the stump of the lower mast; it is also steadied by a double cap G, separately shown in fig. 4, on which it fids; as it does finally on the top. The topgallantmast H is fixed to the topmast F by the top and cap in the usual manner. The figures 3 and 4 show the caps separated from the masts, and are the only things necessary to be made for the purpose; and the object of the cap, fig. 4, is to steady and prevent any wringing of the lower jury mast, and to fid the topmast whenever it is reefed. In the proposed method of securing the shrouds of ships in general, I represents the screws with loop holes for the shrouds to be lashed to, and K the screw nuts, which by means of a proper wrench may be easily turned, so as to tighten or slacken the shrouds at pleasure.

The

The other side of the ship is shown fitted up in the common way, that the contrast may be observed.

Fig. 2 is a side view of the mast and rigging, in which the same letters refer to the same parts.

VIII.

On a native Arseniate of Lead. By the Rev. WILLIAM GREGOR. *Communicated by* CHARLES HATCHETT, *Esq. F. R. S.**

Native arseniate of lead not known,

§ 1. THAT the oxide of lead and the arsenic acid might be found in the state of natural combination, is a supposition highly probable, from the strong affinity which subsists between these two substances. But the existence of such a compound has not, as I conceive, hitherto been established by such proofs, as entitle it to be ranked among the *decided cases* of mineralogical science. I trust, therefore, that the observations, which I have the honour of submitting to the Society, on a new † ore of lead lately discovered in the county of Cornwall, so justly celebrated as well for the variety as for the richness of its mineral productions, will not be deemed superfluous.

till lately discovered

in a copper mine in Cornwall.

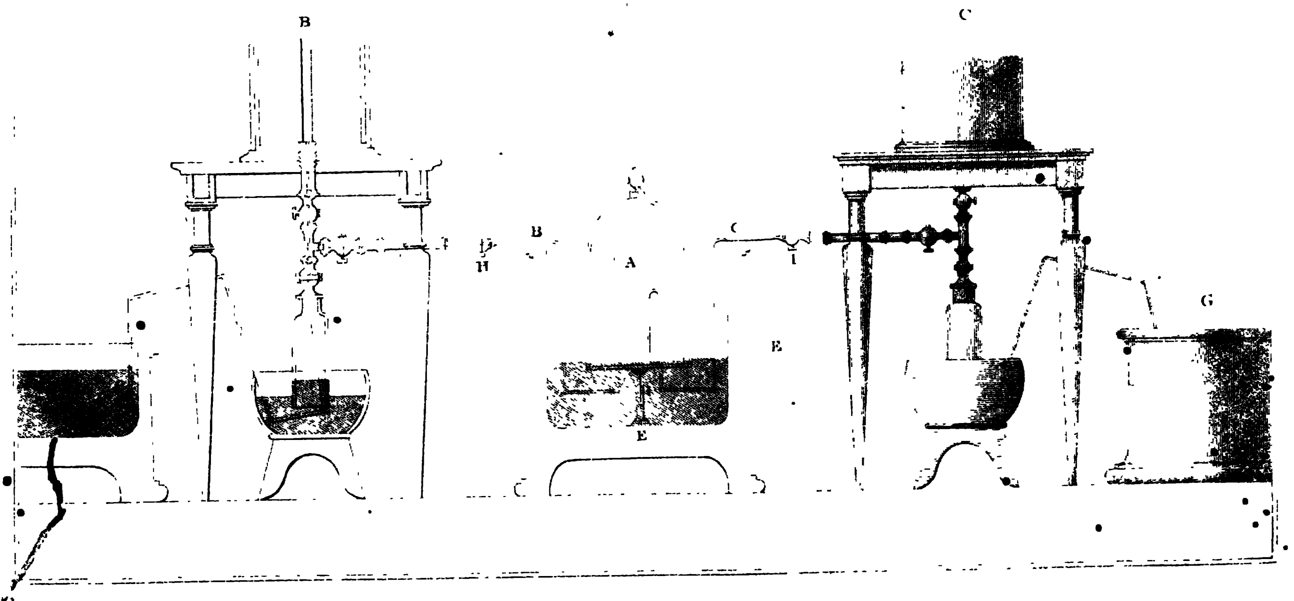
This mineral was raised in the mine called Huel-Unity, a very rich copper mine, in the parish of Gwennap. According to the information with which I have been favoured by Mr. William Davey, a very intelligent and experienced miner in that district, it was found in a lode south of Huel-Unity principal lode, at the depth of fifty fathoms below the surface, which lode underlay about two feet in the fathom south. At the depth abovementioned, this lode fell in, or formed a junction with another small lode or vein to the south, and when the junction took place, this lead ore

* Philos. Transact. for 1809, p. 195.

† It is new at least to the miners in Cornwall; nor was there, previously to this discovery, any ore resembling it to be found in that splendid collection of minerals, which my valuable friend Philip Rashleigh, Esq., has so liberally formed, and as liberally employed in the promotion of science.

was

Mess.rs Allen & Pepys. Apparatus for Respiring Gases.



Mr Miller's apparatus for raising Bodies of Persons sunk under Water.

Fig. 1

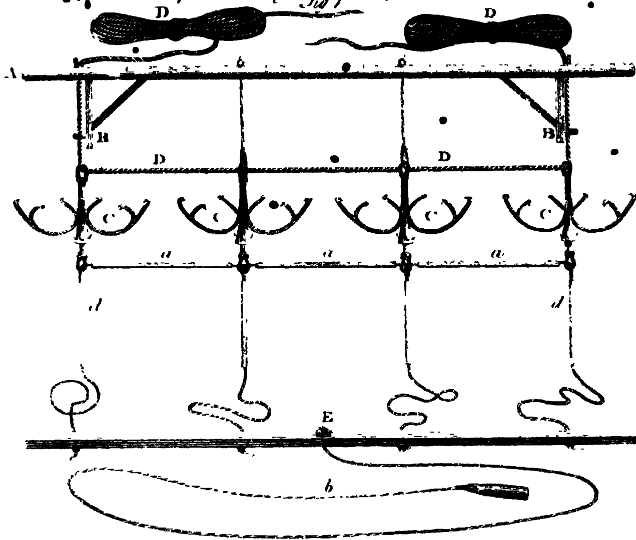


Fig. 2

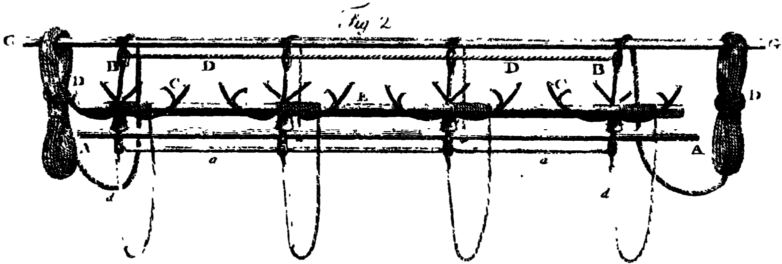


Fig. 3

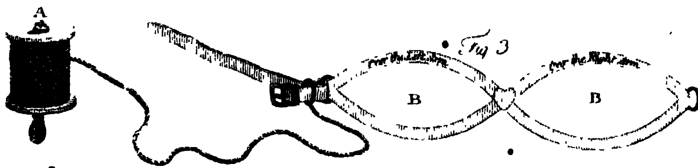
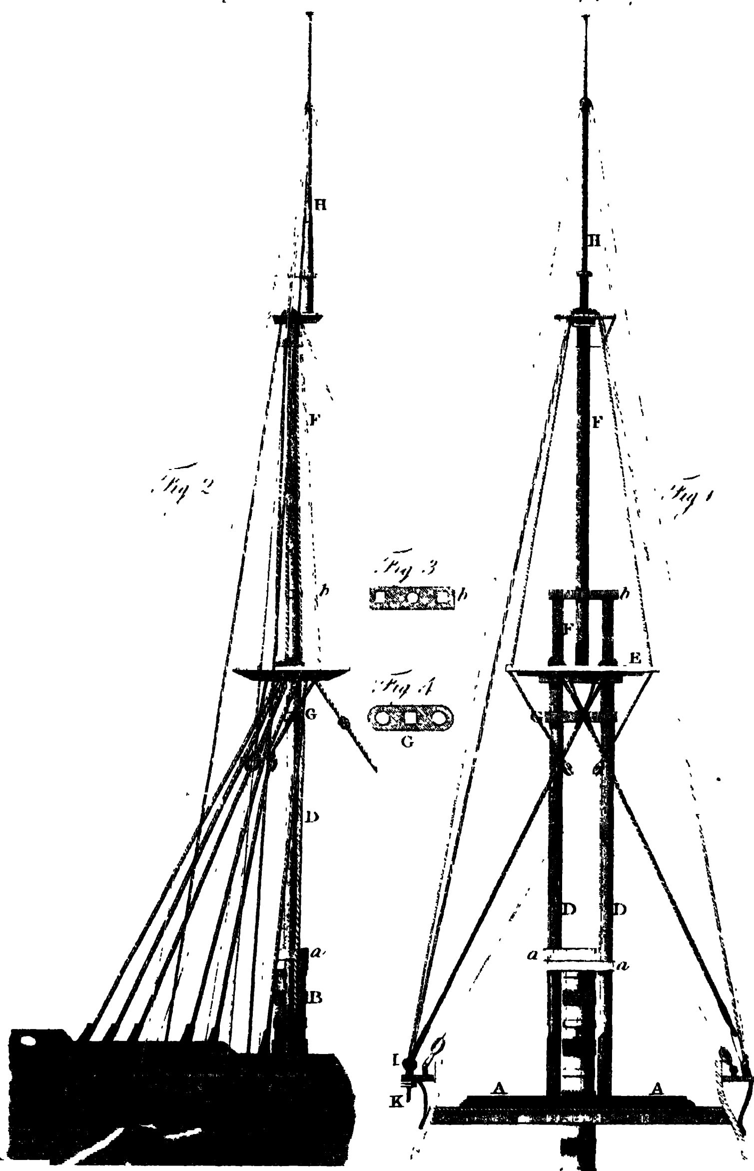


Fig. 4.



Captn. Bolton's method of rigging his Jury Mast, and setting up a Ship's Lower Rigging



was found. The veins of it are, in general, from six to ten inches wide, and they diverge on going west. Some particles of this lead ore have been found in the southern part, after the separation of the lodes; but the northern lode does not contain any, until the junction takes place. This ore is intermixed with some native copper, very rich gray copper, and black copper ore, and some is mixed with quartz. The walls of both veins are killas.

Description of this mineral.

§ 2. This mineral is regularly crystallized. The form of its most perfect crystals is a hexaedral prism; they are of different sizes, from one tenth of an inch in diameter, to the size of a hair. The longest which I have seen do not exceed three tenths of an inch in length: these terminate in a plane, at right angles, with the axis of the prism; but the crystals of a smaller size are frequently drawn out into a very taper acumination, which appears to be a six-sided pyramid. A number of smaller crystals are often closely packed together in bundles, which are bent in different directions, and terminate in a point. The larger crystals either stand alone, or adhere, on their lateral planes, to the gangue, or are confusedly matted together in a mass.

Form of its crystals.

Some of them are hollow, as if an internal nucleus had been destroyed: and sometimes this internal nucleus overtops the external laminæ. The gangue is a white quartz, which frequently exhibits on its surface the appearance of a partial decomposition.

The red octaedral copper ore, and the copper into which that ore passes, are often intermingled with the crystals of this lead ore and inbedded in them.

Colour.

The colour of these crystals consists of a variety of tints of yellow. Some are of a beautiful wine yellow resembling the Brazilian topaz: this, in the greater number of specimens, passes into a delicate Isabella-colour: while, in other cases, we have the honey-yellow mingled with brown hues of different intensities: so that we meet with crystals resembling dark brown sugar-candy, or common resin.

Transparency.

Some of the crystals are beautifully transparent, while others possess this quality in part only, at their extremities, or in inferior degrees throughout their whole length.

The

Lustre. The external lustre, in some specimens, is vitreous; in others, resinous: but in some instances their surface is partially covered by tender and delicate filaments of a silky lustre. These filaments are sometimes found in a separate state loosely adhering to quartz; and they form a variety of this fossil.

Hardness. The crystals vary as to hardness. The angular fragments of the most transparent are sufficiently hard to scratch glass.

Tenacity. This mineral is easily reduced to powder, which has the appearance of pounded resin; it contracts a yellower tint by long exposure to the air.

Spec. gravity. The specific gravity of the purest crystals, taken at the temp. of 50° Fahrenheit, was 6·41.

Action of the blow-pipe on it, § 3. A fragment of crystal, exposed to the flame of the blowpipe in a gold spoon, melted into a brownish yellow mass, which on cooling did not assume any angular figure. It remained in a state of ignition apparently unaltered; but when a piece of it was exposed to the flame on charcoal, a rapid decomposition took place, arsenical vapours were extricated, and globules of a metal, possessing the common properties of lead, were left behind.

and of nitric acid. This mineral, in a state of fine powder, is soluble in nitric acid, even without the aid of heat. Care, however, must be taken, that it does not concrete into lumps. The vessel therefore which contains it must be frequently shaken, and the nitrate of lead produced must be, from time to time, dissolved in water, and poured off from the residuum. The process of solution is, however, accelerated by a digesting heat. Some silica remains, which, as the quantity of it is variable according to circumstances, appears not to be an essential ingredient of this fossil.

Nitric solution. The nitric solution is colourless; its transparency is not disturbed by nitrate of barytes. Nitrate of silver renders it turbid, and a small quantity of white curdly matter is deposited. Sulphuric acid and the liquid sulphates produce copious precipitates of a white heavy matter. If the fluid be poured off from this subsided matter, and it be freed from the superfluous sulphuric acid, by the means of nitrate of barytes, it will yield, on the affusion of liquid nitrate

nitrate of lead, an abundant white precipitate, which, urged by the flame of the blowpipe on a support of charcoal, resolves itself into reduced lead and arsenical vapours.

Inferences. These preliminary experiments led me to the probable conclusion, that this fossil chiefly consisted of oxide of lead, arsenic acid, and a small quantity of the muriatic acid.

Analysis. § 4. A. 1. Fifty grains, carefully selected from crystals of a pale Isabella-colour, were reduced to a fine powder, and exposed to a low red heat for about an hour. Their weight was diminished by 0·15 of a grain.

2. The yellowish powder was now transferred to a vessel of pure silver, and mixed with a lixivium containing fifty grains of potash, prepared by the means of alcohol; a quantity, which I had previously ascertained to be sufficient to effect a complete decomposition of this mineral. The ley was gradually evaporated to dryness in a sand-bath. The soluble part was extracted by distilled water, and poured off from a yellowish white matter, which was sufficiently edulcorated (*a*).

3. Liquid nitrate of ammonia was now dropped into the alkaline fluid, as long as it produced any cloudiness: the clear fluid was now decanted from a small quantity of white matter, which had subsided, and rendered acid by nitric acid; ammonia, added to excess, produced a slight turbidness. These precipitates, after sufficient edulcoration, were added to the yellowish white residuum (*a*).

4. The liquid was now rendered slightly acid by nitric acid, and a solution of nitrate * of lead in distilled water was dropped into it, as long as it separated any precipitate. The clear fluid was poured off, and evaporated nearly to dryness, and a small quantity of white matter, thus obtained, was added to the former precipitate, which dried, and

Oxinitrate of lead. * If the colourless liquid *oxinitrate* of lead be dropped into a dilute solution of arsenic acid, or of arseniate of potash acidulated by nitric acid, no immediate precipitation of an arseniate of lead is produced; but crystalline grains are, after a time, gradually deposited at the bottom of the vessel. But liquid *nitrate* of lead causes an immediate and abundant precipitate from these same dilute solutions. These two combinations therefore must be different.

exposed.

Analysis. exposed to a low red heat, weighed, while still warm, 40·8, which, according to the proportion of 33: 100, established by Mr. Chenevix, implies, 13·46 of arsenic acid.

5. The superfluous lead was now separated from the fluid by sulphate of soda, and filtered off. Ammonia precipitated a minute portion, of flaky matter; it weighed, after ignition, 0·2 of a grain; it consisted of silica and oxide of lead, and must be attributed to the nitrate of lead employed.

B. 1. The yellowish white residuum (*a*) (A, 2.) was dissolved without effervescence in nitric acid, except a minute portion of silica, which, after ignition, = 0·28. A white heavy matter was thrown down from this solution, by liquid sulphate of soda. The clear decanted fluid was evaporated to a small volume, and sulphate of soda produced a farther separation of white matter. It was sulphate of lead, which, after exposure to a low red heat, and weighed, while warm, = 47·5; which, upon the supposition that one hundred parts of sulphate of lead contain 69·74 of lead + 3·48 of oxigen, are equivalent to 34·77 of oxide of lead.

2. The fluid, now freed from lead, deposited, on the affusion of ammonia, a greenish matter, which, after ignition, became red, and = 0·033 of a grain. It was oxide of iron.

C. 1. One hundred grains of larger crystals, some of which were hollow, and the surfaces of which were slightly and partially covered with silky filaments, treated in the same way, yielded 95·283 of sulphate of lead, equivalent to 69·76 of oxide; and 80 of arseniate of lead, which indicate 26·40 of arsenic acid. The oxide of iron, in this case, amounted to only 0·05 of a grain, and the residuary silica was in too small a quantity to be weighed.

2. I have endeavoured to decompose this fossil by boiling it to dryness in a solution of four times its weight of the purest subcarbonate of potash, and exposing the dry mass, for a very short time, to a low red heat; but I found, that only a part of the arsenic acid had united to the alkali; the larger portion of it was detected in the nitric solution of the residuum; but the relative proportions of the oxide and the acid were found to correspond almost exactly with the foregoing statement of them.

3. I

3 I found also, that carbonate of ammonia precipitated this mineral, *in an unaltered state*, from its solution in nitric acid; as no arsenic acid had united with the precipitant. The solution of the nitrate of ammonia was evaporated to dryness, and exposed to a red heat in a platina crucible; but nothing was left, except a slight trace of oxide of lead. We may infer from this the absence of both the fixed alkalis.

Analysis.

4. I found in one specimen only of this fossil any notable difference in the relative proportions of the oxide of lead and of the acid, to which it is united. It consisted of crystals confusedly matted together in a more compact mass, than this fossil generally assumes. One hundred grains were dissolved in nitric acid; the marine acid was separated by nitrate of silver, and any redundant silver by muriate of ammonia. The lead was separated by sulphuric acid, and the superfluous portion of that acid by nitrate of barytes, and the arsenic acid was combined with oxide of lead by the affusion of nitrate of lead. The muriate of silver = 9·8; the sulphate of lead = 97·6, and the arseniate of lead = 72, equivalent to 1·63 of muriatic acid, 71·46 of oxide of lead, and 23·88 of arsenic acid, respectively. The quartz = 0·35, and the oxide of iron 0·2 nearly.

Variety.

Another portion taken from the same specimen, treated with an alkali, gave very nearly a similar result.

D. It will now be necessary for me to speak concerning an ingredient of this fossil, which I may have seemed to overlook. I mean the muriatic acid: I have found some difficulty in ascertaining the proportion, which it bears to the other constituent parts, and from a cause, which I did not suspect. I considered, that the only sure mode of determining this point was to have recourse to nitrate of silver, which might effect a direct separation of the marine acid from the nitric solution of this fossil. But I found, in many experiments upon given quantities of this mineral, that the results, which I derived from this most valuable chemical test, were variable and uncertain.

Nitrate of silver as a test of muriatic acid varied in the quantity it indicates.

At last, I was enabled to trace the errour and uncertainty up to two sources. In the first place, I found that the muriate of silver was more abundant in the cases, where I employed

1st source of errour.

employed a vessel with a long neck for the solution, and *did not expose it to heat.*

I concluded therefore, that, when the process was conducted under different circumstances, the predominating *mass* of nitric acid produced its effect, and volatilized a portion of the muriatic.

2nd source of errour.

Another source of errour I found in the following anomalous circumstance, *viz.* a simultaneous precipitation of a portion of arseniate of lead takes place with that of muriate of silver. Whatever combination this may be, it is a weak one, and may be severed by nitric acid, which dissolves the arseniate and leaves the muriate; or by ammonia, which takes up the muriate, to the exclusion of the arseniate.

The conclusion, to which many experiments have led me, is this, that the muriate of silver, produced in the nitric solution of one hundred grains of arseniate of lead by nitrate of silver, amounts to about 9·5.

Proof that the acid in this mineral is the arsenic.

E. In order to prove, that the acid, which is combined with the oxide of lead in this mineral, *is the arsenic acid*, and that it is not combined with phosphoric, I decomposed some of its acid, which had been combined with lead in the foregoing experiments, by means of sulphuric acid, and filtered off the sulphate of lead. The fluid which passed through the filter was evaporated nearly to dryness, and it assumed the appearance of crystalline grains. Some of it was exposed to the flame of the blowpipe in a gold spoon; at first it became like a white dry powder, which melted before an increased heat: placed on *charcoal* and ignited, it was totally dissipated in arsenical fumes.

Some of it was dissolved in water, and dropped into liquid sulphate of titanium, a white precipitate was produced: combined with soda, it precipitated silver from the nitrate of silver, of a brick colour. It precipitated mercury from its nitrate, of a yellowish colour, which afterward became reddish. This precipitate, exposed to the flame of the blowpipe on charcoal, exhibited the same phenomena as arseniate of mercury.

Fallacious mode of distinguishing the phosphoric

I precipitated magnesia from its muriate, and redissolved it by carbonate of ammonia, perfectly saturated with carbonic acid. I divided this liquid into two portions, and dropped

dropped into *both* a solution of the combination of the acid of this mineral and soda. No precipitate was produced. I dropped into *one* of the vessels some liquid phosphate of soda, and a separation of saline matter was instantly produced. I soon, however, found, that this mode of distinguishing the phosphoric from the arsenic acid could not be depended upon. For in the other vessel, into which no phosphate of soda had been dropped, in a short time saline tufts made their appearance, and an abundant deposition of saline matter was formed. I found also, that if the solution had been more concentrated, the precipitation would have immediately taken place.

acid from the arsenic.

On making a comparative experiment with arsenic acid, I found, that it forms a triple salt with ammonia and magnesia, analogous to the phosphoric salt described by Dr. Wollaston. The figure of the arsenical salt, as far as I could determine it from a confused crystallization, is a triedral prism.

We are therefore, I think, authorized from the experiments herein detailed, to conclude, that the fossil, which is the subject of this paper, is arseniate of lead, and that, if we state that the relative proportion of the constituent parts of it are in one hundred as follows, we shall not be far from the truth:

Oxide of lead	- -	69·76
Arsenic acid	- - -	26·40
Muriatic acid	- -	1·58

Component parts of the mineral.

The silica and the oxide of iron, which account for a portion of the loss; and the alumina and copper, which are sometimes found in an analysis of this fossil; I do not conceive to be essential to it.

The existence of a minute portion of muriatic acid as a constant ingredient of it is a curious fact: and it is still more curious, when we consider it in connexion with the analogy, that in this particular it maintains with the natural phosphates of lead.

Curious fact.

IX. *Experiments*

IX.

Experiments on the Combination of Phosphorus with Metals and their Oxides in the humid Way; to which is added the Examination of a Gas arising from a peculiar Decomposition of Alcohol: by Mr. THEODORE DE GROTTHUSS *.

Phenomena exhibited by solution of phosphorus in alcohol.

§ 1. IT is well known, that the surface of water becomes luminous in the dark, when spirit of wine, in which phosphorus had been digested, is poured on it. Another curious appearance may be exhibited, by half filling a small phial with this alcohol, and putting it in a dark place, at a temperature of about 64° R. [176° F.] Even before the liquid begins to boil, a flame makes its appearance at the mouth of the phial, and reaches sometimes to the height of five or six inches, but is incapable of heating or setting fire to any thing. You may hold your finger in it some time without danger, and without feeling any heat. This flame exhibits to us an intermediate state between a simply luminous vapour, as is that of phosphuretted nitrogen, and an inflamed vapour, which in fact differs from it only by more rapidly decomposing the oxigen gas of the atmosphere.

Metallic phosphurets produced by it.

§ 2. Phosphuretted alcohol, poured into solutions of gold, silver, mercury, and copper, suddenly reduces these four metals, and precipitates them in the state of real phosphurets. Two drops of nitrate of silver, or of mercury, in a drinking glass full of distilled water, will be rendered very sensible by the addition of a few drops of phosphuretted alcohol, which immediately occasion a dark precipitate. The phosphuret of gold thus obtained had a considerable resemblance to the purple powder of Cassius, but differed from it in being of a deeper colour like that of indigo. When applied on porcelain, this appears neatly gilt on being exposed to the flame of the blowpipe, and a smell of phosphorus is emitted.

[illegible]huret of

Phosphurets of silver, mercury, and copper.

The most essential generic characters of the other phosphurets are: 1, to dissolve in nitric acid, with the evolution of nitrous gas, after which an insoluble phosphuret

* Annales de Chimie, vol. LXIV, p. 19.

remains:

remains: 2, to emit a beautiful phosphorescent light in the dark, when thrown on a hot iron, and leave a metallic pellicle as a residuum.

Their specific characters are easily determined, first by the metal left after the action of the fire, and secondly by that found in solution in the nitric acid.

Their formation may be explained in the following way. The spirit of wine impregnated with phosphorus quits it, as soon as it is mixed with the solution of either of the four metals mentioned above, and unites with the acid, producing ether. The metallic oxide, being set free, gives up its oxigen, either to the hidrogen of the alcohol, or to a small portion of the phosphorus, the greater part of which seizes on the metal in its pure state. This explanation results from the products of the two liquids, the phosphuret that falls down, and the ether that remains in the liquid, the fragrant smell of which is perceptible the moment after the operation is effected.

Their formation explained.

§ 3. A very singular phenomena exhibited in this experiment is the agitation, which the metallic particles display. If, for instance, a few drops of phosphuretted alcohol be dropped into a solution of gold contained in a shallowish vessel, immediately we perceive currents take place throughout the whole of the liquid, and particles of the revived metal attracted in all directions. Sometimes they dart with impetuosity toward the sides of the vessel; sometimes a centrifugal force appears to whirl them round a common centre; and these commotions are always carried on with such velocity, that the eye can scarcely follow them. This exhibition is frequently rendered more pleasing by the fine prismatic colours displayed on the brilliant particles in motion. No doubt this decomposition of the light must be ascribed to a very thin stratum of some heterogeneous substance adhering to the metal, as in those coloured rings which Newton observed between convex lenses. I satisfied myself by a very simple experiment, that these movements were owing to the evaporation of the alcohol; for they ceased as often as I placed the vessel before a window, where the temperature was that day at 0 [32°F.]; and the moment I took it thence, and brought it near a gentle fire, the commotions began anew with great rapidity.

Singular agitation of the liquid.

Prismatic colours.

The agitation owing to evaporation.

 § 4. The

Precipitates of tin, lead, bismuth, antimony, and platina, by phosphuretted alcohol.

§ 4. The precipitates produced by phosphuretted alcohol in solutions of tin, lead, bismuth, antimony, and platina, do not differ from those obtained by means of alkalis, and in some even by alcohol; except that the former are mixed with a little phosphorus, the separation of which hastens that of the oxide. The phosphorus is simply mixed with them however, for the colour of the precipitate is not altered by it; but we shall see presently, that this is greatly changed, as soon as a chemical combination of it with the oxide takes place.

Very different when the phosphuretted alcohol contains an alkali.

To effect this I boiled two parts of phosphorus with one of caustic potash in about six parts of rectified alcohol, the whole being contained in a small glass phial *. The greater part of the phosphorus was not attacked, and could be used in the subsequent experiments. After cooling, the liquid let fall an oxide of phosphorus of a very fine red colour, and the supernatant fluid became perfectly clear. It was this that was employed in the following experiments.

Nitrate of lead precipitated by this compound

A portion of this phosphuret of potash in alcohol, being poured into a solution of crystals of nitrate of lead in water, precipitated a substance of a fine orange colour. Commotions took place throughout the whole of the liquid, and in a few moments black, shining, crystalline grains separated; which on examination comported themselves as metallic lead retaining a little phosphorus. The same solution of nitrate of lead, one portion of which I treated with spirit of wine impregnated with phosphorus simply, and another with caustic alkali, yielded in each case a white precipitate; while alcohol impregnated with phosphuret of potash immediately produced with it a very bright orange. I then attempted to give this fine colour to the white oxide thrown down by the alkali, by adding to it when separated phosphuretted spirit of wine, but in vain: which proves, that the orange-coloured substance is formed only at the moment when the phosphorus and oxide meet in a state of extreme tenuity favourable to their intimate union.

Properties of the precipitate.

§ 5. This orange precipitate easily loses its colour, some-

* An examination of the gas evolved in this process will be found toward the end of this paper, at § 11.

times

times growing darker, at others becoming singularly white. To avoid the latter accident, which is owing to the volatilization and combustion of the phosphorus, it should be washed and dried cold. It then retains its brown red tint, and has the following properties. 1. Rubbed between the fingers it yields a smell of garlic. 2. Thrown on a hot iron, it emits the same smell, accompanied with a very beautiful phosphorescence, and leaving a residuum of phosphate of lead, fusible before the blowpipe, mixed with some white oxide. 3. *Acids expel from it no aeriform bubbles, but develope a nauseating and unequivocal smell of phosphuretted hidrogen.* 4. Nitric acid dissolves the greater part of it, but *without the least extrication of nitrous gas;* and the residuum is nothing but phosphorus.

Its nature.

From all these properties I conclude, that this substance is composed of oxide of lead, phosphorus, and a little hidrogen; or that it is a hidroguretted phosphuret of oxide of lead*.

Nitromuriate of antimony precipitated.

§ 6. Water, alkalis, and spirit of wine in which phosphorus has been digested precipitate the nitromuriate of antimony white; but alcohol impregnated with phosphuret of potash occasions in it a fine brown precipitate, which, being washed and dried without heat, remains unaltered, and has a considerable resemblance to kermes mineral. The mode in which this powder is formed; its phosphorescence by the action of caloric; its quiet and partial solution in muriatic acid, which produces with it the alliaceous smell; and the residuum of phosphorus; joined with the property of not decomposing nitric acid; evidently prove, that it is hidroguretted phosphuret of oxide of antimony.

The compound precipitates all metals, but does not always form phosphurets with their oxides.

§ 7. Though alcohol impregnated with phosphuret of potash precipitates all metallic solutions, it does not always form phosphurets of their oxides. In some cases the precipitates are oxides simply mingled with phosphorus, as occurs in the solutions of tin, zinc, cobalt, and manganese: in others they are phosphurets containing the metal in a pure

* If this phosphuret have been some time exposed to the air, its oxide absorbs carbonic acid, and the nitric acid then extricates a gas, which the operator might at first be tempted to take for nitrous gas, but it has neither its smell nor its rutilant appearance.

 state,

state, and consequently decompose nitric acid; as those formed in solutions of silver, mercury, copper, and bismuth. I believe however, that beside the two phosphurets of oxides I have mentioned, others might be produced by repeating our trials, and varying the proportions of phosphorus and alkali, for these are of the greatest importance in preparing the two species I have mentioned. If, for instance, the precipitant contain too much phosphorus, or too much alkali, the precipitate will be white, both from the nitrate of lead, and from the nitromuriate of antimony; and we cannot obtain the orange precipitate in the first case, or the brown in the other, but by adding to the alcohol whichever of the two substances is deficient.

The proportions of the precipitant important.

The action of sulphuret of potash exhibits a striking analogy with this. This sulphuret precipitates the nitrate of lead *red;* but, if it contain too much alkali, the precipitate is *white;* and if the sulphuret be sufficiently hidroguretted, it is *black*. In the latter case however the precipitate may be made red by adding an excess of acid to the metallic solution, for this extricates the hidroguretted sulphuret.

Sulphuret of potash analogous in its action

§ 8. Mercury dissolved in nitric acid furnishes with the alcaline phosphuretted alcohol * a precipitate, that fulminates loudly when struck. As we do not always succeed in obtaining it equally fulminating in this way, it appears to me a more certain method, to prepare it by mixing phosphuret of mercury, obtained by means of phosphuretted alcohol (see § 2), with small dry crystals of nitrate of mercury.

Fulminating mercury.

§ 9. Though lead dissolved in acids is not separated in the metallic state by alcohol impregnated with phosphorus †, it is when its oxide is held in solution by alkalis. We have only to let fall a few drops of this precipitate into an alkaline plumbate, when the metal and the phosphorus will immediately unite, and separate in the form of small, black, brilliant, granular crystals. The presence of the alkali in this

Alkaline solution of lead precipitated metallic.

* That must be employed, which gives an orange or red precipitate with solutions of lead; and it is essential, that the precipitate be washed only with cold water.

† This alcohol must not be confounded with that which contains, beside phosphorus, alkali likewise.

this case occasions the formation of a phosphite, and by this the attraction of the phosphorus for the oxigen is increased.

These observations suggest, that metals may be classed by their mode of comporting themselves with phosphuretted alcohol; and perhaps this system of division would be less equivocal than any other.

Classification of metals.

§ 10. If we write on a piece of paper with solutions of gold, silver, mercury, and copper, and put the paper into a phial containing a bit of phosphorus, every letter will appear in a few minutes with the lustre of the original metal; and its brightness may be afterward increased by rubbing it with a polished substance, as glass, &c. It is sufficient even to leave the phosphorus a few minutes in the phial, after which it may be taken out; for the vapour emanating from it, though invisible, retains the revivifying power, and gives the writing the metallic lustre.

Sympathetic metallic inks.

Alcohol impregnated with phosphorus affords an excellent means of depriving water of the oxigen gas it commonly contains. For this purpose we have only to let fall a few drops into the water, and expose it to the light of the sun: the phosphorus will be oxided, without passing to the state of an acid, and the water may then be filtered, to render it clear.

Mode of freeing water from oxigen gas.

Decomposition of Alcohol at no very high Temperature.

§ 11. When phosphorus and caustic fixed alkali are digested or boiled in rectified spirit of wine, this liquid is decomposed, and gives rise to the formation of water and of phosphocarburetted hidrogen gas, which may be collected in the hydropneumatic apparatus, and in which I have found the following properties.

Decomposition of alcohol at a low temperature

a. This gas is invisible, and diffuses an alliaceous smell resembling that of phosphuretted hidrogen gas; with which however it cannot be confounded, as it does not take fire spontaneously, either in the open air, or when mixed with oxigen gas. I endeavoured to increase the proportion of phosphorus in it, by diminishing that of the alcohol and alkali; and then placed the gas in a heat of 70° R. [189.5° F.], yet it did not inflame on contact with the atmosphere.

Properties of the gas obtained.

b. When

b. When kindled by the flame of a candle, its burning is attended with vapours of phosphorus, part of which is precipitated in a concrete form on the sides of the vessel containing it.

c. It inflames with detonation, when mingled with oxigen gas.

d. Nitrous gas occasions no alteration in it: but if oxigen gas be added to a mixture of these two, a bright light appears, which is followed by a violent and very dangerous detonation. In the experiment I made on a fourth part of a cubic inch of the inflammable gas, enclosed in a glass tube which I had purposely selected for its thickness, the tube was broken to pieces, and the fragments flew up to the cieling of the room, where they made deep marks.

e. If three measures of oximuriatic acid gas be introduced in separate portions to one measure of this inflammable gas in a tube over water, it will take fire at the introduction of each portion, and burn with a very fine green light. The mixture expands at the commencement, and afterward diminishes prodigiously. All this goes on quietly, and there is no danger in the experiment, if we take care to use a pretty long tube.

f. It is injurious to the germination of plants, as appears from the following experiment. Having placed three parcels of the seeds of water cresses *(sisymbrium)*, moistened with water, under three jars, one of which contained oxigen gas, one atmospheric air, and the third this inflammable gas, I left them thus four and twenty hours. I then found, that the seeds in the first jar had begun to germinate, for they were soft and viscous; those in the second jar were less so; and those in the third not at all. The seeds were sown separately at the same time in good mould, and the celerity of their vegetation was in the same order. The seeds that had been in oxigen gas came up in three days; those that had been in atmospheric air, in four; and those that had been in the inflammable gas, not till the end of the sixth.

Analysis of it.

g. To find the nature of this gas, I introduced fifty measures of it, and a hundred of oxigen gas, into Volta's eudiometer previously filled with limewater. After the inflammation

flammation of this mixture by the electric spark, the absorption was found to be at first eighty; no phosphorus was deposited; and on washing the residuum six measures more were absorbed. The sixty four remaining were nearly pure oxigen, for they were almost wholly absorbed by being fired repeatedly with proportional additions of hidrogen gas. The precipitate produced in the lime-water, being treated immediately with nitric acid, dissolved in it with effervescence: but it did not consist entirely of carbonate of lime; for caustic ammonia, poured into the nitric solution, separated a great part of it, namely the phosphate of lime. Hence we may conclude, that fifty measures of this gas require thirty six of oxigen gas to burn it completely; and that the products of this combustion are water, phosphoric acid, and carbonic acid: consequently it is phosphocarburetted hidrogen gas *.

Its nature.

§ 12. The liquid remaining in the matrass, in which alcohol has been boiled with phosphorus and caustic fixed alkali, contains no phosphate, as we may satisfy ourselves by adding lime-water to it: but if the boiling have been long continued, phosphuretted hidrogen gas comes over toward the end, the bubbles of which take fire spontaneously on coming into contact with the air; and then the remaining liquid contains more water than alcohol, and a little phosphate. Hence it appears, that the presence of caustic alkali, more soluble in water than in spirit of wine, and the tendency of phosphorus to unite with hidrogen and carbon, give rise to the phosphocarburetted hidrogen gas, and the conversion of the alcohol into water, by subtracting the whole of its carbon, and the greater part of its hidrogen. It is remarkable, that the phosphorus, which remains after long boiling in the alkaline lixivium, remains in a fluid state at a temperature of 10° [54·5° F.] or less; but on throwing it into water, or into a vessel of any kind, it becomes

Residuum of the decomposed alcohol.

State of the phosphorus left.

* Trommsdorff having strongly heated phosphoric acid with charcoal, obtained a gas, which he found to be a triple compound of hidrogen, phosphorus, and carbon. (See Van Mons's Chemical Journal, vol. II, p. 213 and 225.) There is every reason to presume, that his gas was the same as that I have examined, though the methods employed to obtain the two were totally different.

A similar gas procured in a different way.

comes solid, and assumes the colour and transparency of the finest amber*.

§ 13. I cannot conclude this paper without mentioning some experiments I undertook, during my residence at Rome, on a species of glow-worm, the *lampyris italica* Lin.; prodigious numbers of which are found in the summer in all parts of Italy.

Experiments on the light of the Italian glow worm.

The light of this insect continues some time, when it is kept under water. It does not appear to be diminished for the first quarter of an hour, and is some hours before it ceases entirely. If oil be used instead of water, it is extinguished more speedily, the light ceasing in fifteen or twenty minutes. It is extinguished equally in hidrogen gas, carbonic acid gas, and in nitrous gas extricated in the hydropneumatic apparatus. If the insect be withdrawn from these gasses soon after its light is extinguished, its phosphorescence revives by the mere contact of the air; but when it will not revive even in oxigen gas, there is a mode of reproducing it with peculiar lustre. This consists in placing the insect, whether living or dead, in the rutilant fumes that arise from the mouth of a phial filled with nitrous acid. As soon as these vapours touch the abdomen of the insect, this becomes luminous, and diffuses a greenish phosphorescence, which speedily increases, till it becomes of a dazzling brightness; and afterward diminishes in proportion as it increased, till it entirely ceases. This beautiful phenomenon continues about a minute only; and though I have tried various means, I could never revive the phosphorescence after its extinction in nitrous vapour. If oxigen gas, or even atmospheric air, be passed up into a phial containing the insect and nitrous gas, the light will soon reappear with more vividness than in its natural state.

Fulminating lead

P.S. After this paper was sent to the editors of the Annales de Chimie, I found a new property in the phosphuret of oxide of lead, which I had not observed before. Having

* I have noticed this phenomenon with soda in particular, and I had added gradually a larger proportion of this alkali than that I have mentioned of potash, § 4.

slightly

slightly boiled five parts of phosphorus with one of pure soda in rectified alcohol, I poured this liquid still hot into a solution of crystals of nitrate of lead in water. The precipitate I removed from one piece of filtering paper to another, so as to deprive it quickly of its moisture, without allowing the oxigen of the air to act on it *. In this state it was of a dark colour, and possessed all the properties mentioned in § 5; but it had likewise that of detonating briskly, when the smallest quantity folded in paper was struck with a hammer. When the paper was opened after the experiment, I found it coated with lead completely revived. I also found, that touching it with a drop of sulphuric acid was sufficient to set it on fire.

X.

Report made to the Physical and Mathematical Class of the French Institute, on a Burning Mirror presented to the Class by Mr. PEYRARD †.

Burning glass of Archimedes.

MR. PEYRARD, who has just published an elegant translation of the Works of Archimedes, was naturally led to reflect on the means, which that great geometrician is said to have employed, to burn the fleet of Marcellus before Syracuse. Both the ancients, and the authors of the middle age relate, that he used a burning mirror; but none of them enter into the particulars sufficiently, to give us an accurate idea of his process. Anthemius, who built the church of Saint Sophia, at Constantinople in the sixteenth century, and appears to have been a very intelligent architect, invented an assemblage of plane mirrors, to produce the same effect as that of Archimedes. Since that time Kircher, who perhaps was unacquainted with the works of Archimedes, thought of something similar. Lastly Count de Buffon constructed a burning mirror, composed of a hundred and sixty eight plane glasses; and the experiments,

* The temperature that day was 15° R. (66° F.).

† Sonnini's Bibliothèque Physico-économique, Nov. 1807, p. 349.

In

in which he employed it, are well known. These three processes, which come to the same thing, are attended with serious inconveniences.

Necessary construction.

For a mirror to reflect to one and the same point the rays of the sun, considered as parallel to each other, the reflecting surface must make part of that of a paraboloid of revolution, the axis of which is parallel to the rays of light, and its focus their point of union. If this mirror were composed of a great number of plane mirrors of moderate size, the plane of each must be parallel to a tangent of the paraboloid at the point where it is cut by the corresponding radius vector. Now in consequence of the motion of the sun the position of the axis of the paraboloid changes with some rapidity. If the form of the mirror therefore be unchangeable, the whole must turn round the focus with the sun, which appears to be impracticable: and if the parts that compose it be movable independent of each other, each of these parts must turn so as to be constantly perpendicular to the right line, that bisects the angle formed by the solar ray and the corresponding radius vector.

Impracticable with glasses fixed in a single frame.

With several glasses movable separately difficult.

It appears difficult to give the component mirrors the movement in question by means of a machine, less perhaps because the change in the sun's declination would render this machine complex, than because the expansion of the metallic rods, used for imparting the motion, would change in a perceptible and unforeseen manner the direction of the component mirrors; and because the action of the machinery would impart to each mirror a vibratory motion, that would keep the image in perpetual agitation.

Each glass managed by a different person.

There remains no other reasonable way therefore of composing a burning mirror of several plane mirrors, but by entrusting each of the latter to an individual, charged with keeping it in the proper position for reflecting the image of the sun to a determinate point, varying the position agreeably to the motion of the sun. But Mr. Peyrard justly observes, that this method is attended with an inconvenience, which must prevent its success. It is easy indeed for a single person, attentive and conveniently placed, to direct to a point the image of the sun reflected from a mirror of moderate size, and to keep it there, notwithstanding the motion

Inconvenience of this.

motion of the luminary. The difficulty would not be very great for three or four persons to do this at the same time. But if fifty, a hundred, or two hundred persons were employed to form a burning focus in this manner, as none of them could distinguish the image he sent from that sent by another, if one of the images alone should deviate from the focus, each of the cooperators would try whether it were his, and hence would arise an agitation and disorder, that would prevent the focus from being formed. This inconvenience Mr. Peyrard purposes to remove in a very ingenious way, by furnishing each of his mirrors with an apparatus not very complex, which we shall proceed to describe.

Contrivance for removing the difficulty.

A small telescope supported on a stand, and furnished with two wires crossing each other in the focus of the glasses, may easily be directed to the point, to which the image is to be conveyed. In this direction it is fixed by two screws. This telescope, without changing its direction, is movable on its axis between two collars, and can be kept in any position round this axis by another screw. On this telescope is fixed the mirror, which it carries with it when it turns round its axis; and which, independent of this motion, is capable of turning round another axis, perpendicular to that of the telescope. The telescope is to be turned on its axis, till the axis of the mirror is perpendicular to the plane formed by the incident and reflected rays, and in this position it is to be fixed by a screw. Lastly the mirror is to be turned on its axis, till the reflected rays are parallel to the axis of the telescope; and then the image of the sun must strike the object at which the telescope points.

First movement.

The two movements here mentioned are executed one after the other, and are capable of considerable precision. With respect to the first, when the axis of the mirror is perpendicular to the plane of the incident and reflected rays, the edge of the frame, which is perpendicular to the axis of the mirror, throws its shadow in a plane parallel to the incident and reflected rays, and consequently parallel to the axis of the telescope. This shadow therefore, or the boundary of the light reflected from the mirror, will cut an index projecting from the telescope in a right line at the same distance,

tance from the axis of the telescope as the edge of the frame is.

Accordingly this right line being traced on the face of the index, for executing the first motion it is sufficient to turn the telescope on its axis, till the shadow of the frame coincides with the right line on the index; which may be done with considerable precision.

Second movement.

For the second movement, it is clear, that, when the mirror is so placed as to have its reflected rays parallel to the axis of the telescope, if in the axis of the mirror, and close to the edges of the frame, a little line of the silvering be removed, the want of silvering will produce a shadow, that will fall on the middle of the right line of the index. This middle point being previously marked on the index, to execute the second movement it suffices to turn the mirror on its own axis, till the shadow of the unsilvered stroke falls on this point; which may be done with the same precision as the former movement.

Each person acts independently of the rest

Thus we see, that every person employed, however great the number, may direct the image he produces to the point assigned for the focus, without troubling himself about what is done by the others, and without being disturbed by their operations. It may be observed too, that the motion of the sun in its diurnal axis is not so rapid, but that one person might attend to ten mirrors near each other, and keep them in the right position, which would greatly diminish the trouble and expense of the process.

We are of opinion therefore, that Mr. Peyrard has carried the construction of burning mirrors composed of several plane mirrors to a degree of perfection, that it had not before acquired, and appears to us to merit the approbation of the class.

Done at the *Palace of the Arts*, *3rd of August*, 1807.

CHARLES,
ROCHON,
MONGE, *Reporters*.

The class approves this report, and adopts its conclusions.

DELAMBRE, *Perpetual Secretary*.

Paris, *4th* of *August*, 1807.

XI. *Analysis*

XI.

Analysis of the Bronzite. By Mr. KLAPROTH*.

A VERY remarkable fossil, found in large masses in the strata of serpentine near Kranbat in Upper Stiria, has been known within these few years by the name of bronzite. The following are its characters, as given by Mr. G. R. Karsten.

Bronzite.

Its colour is a light tombac brown.

Its characters.

It is in masses, disseminated in large pieces.

It has a metallic semibrilliancy.

Its fracture is lamellar, very distinct from simple splitting.

The pieces separated are large grained.

In thin plates, it is translucid: in the mass, opaque.

Where scraped it appears white.

It is semihard, and very brittle.

Its specific gravity is not great. That of the specimen analysed was 3·2.

A. Exposed to a red heat for half an hour its colour was rendered a little lighter, and it lost half a part per cent.

Analysed

B. *a.* 100 grains of bronzite were thickened with a lixivium containing 200 grains of potash, and then kept at a red heat for half an hour. The mass, which had not entered into fusion, was triturated in a mortar, then softened with hot water, supersaturated with muriatic acid, and completely dissolved. Being evaporated to dryness, and then treated with water acidulated with muriatic acid, the silex remained, which, after calcination, weighed 60 grains.

Silex

b. The muriatic solution was neutralized cold with carbonate of soda The precipitate, treated with a boiling lixivium of potash imparted nothing to it. Well washed and heated red hot, 10·5 grs. of oxide of iron remained.

Oxide of iron.

c. The colourless liquid, thus divested of iron, was made to boil, after which a sufficient quantity of carbonate of soda was added to decompose it entirely. The precipitate

* Annales de Chim. vol. LXV, p. 107. Translated from Gehlen's Journal.

pitate obtained, after being strongly heated, consisted of 27·5 grains of pure magnesia.

Magnesia.

C. Sixty grains of bronzite were heated red hot with 300 grains of nitrate of barytes, till the nitrate was completely decomposed. The mass, being triturated, diluted with water, and supersaturated with sulphuric acid, was boiled for some time, and then filtered. The free sulphuric acid having been saturated in great part with ammonia, acetate of barytes was added. The supernatant liquid was poured off, and evaporated to dryness. The residuum was heated red hot, then elutriated with hot water, and the liquid filtered off. This liquid contained a trace of potash, for reddened litmus paper acquired a blue tinge from it after some time; but one drop of nitric acid was more than sufficient to destroy this alkalinity, and give the liquid an acid character.

Component parts.

Bronzite therefore is composed of

Silex	- -	60
Magnesia	-	27·5
Oxide of iron	-	10·5
Water	- -	0·5
		98·5

Occurs disseminated in serpentine.

The bronzite here described is the only instance known of its being found in compact masses; though it is frequently found in little separate pieces in serpentine; for instance, near Teinach, in the Pacher-Alp, in Lower Stiria, at Mount Hradicko in Moravia, at Zellerwalde near Siebenlehn, near Guanabacoa in Cuba, &c.

It is not yet decided whether the schillerstein (schillerspath, schillerblende) in the serpentine of Baste, near Hartzbourg, in the Hartz, may also be classed with it. As the analyses of this mineral yield alumine to the amount of 0·23 according to Heyer, and of 0·18 according to Gmelin, it should be classed with the schillernden hornblende, as has already been done by Mr. Karsten, if this large proportion of alumine be confirmed.

Haüy's diallage,

Mr. Haüy has made a separate species under the name of diallage, in which he has classed the smaragdite as green lamello-fibrous diallage, and the bronzite he gives as a variety of

of this by the name of bronzed metalloid lamello-fibrous diallage. On the contrary he separates from it the Labrador hornblende, of which he makes a distinct species under the name of metalloid, reddish brown, laminar hypersthène.

In my opinion our bronzite cannot be ranked with the emerald, or the diallage, as it is of a different nature; for, according to the analysis of Mr. Vauquelin, the diallage not only contains a little magnesia and alumine, but a preponderant proportion of lime, to say nothing of the chrome. It is distinguished too by melting alone into a scoria before the blowpipe, while the bronzite is infusible.

different from the bronzite.

XII.

Extract from a Letter of Mr. GEHLEN, *on the Analysis of the Kannelstein and Greenland Garnet, on some Metallic Succinates, &c.**

IN my way through Berlin Mr. Klaproth gave me the analyses of the kannelstein, Greenland garnet *(almandine)*, and haarkies (capillary pyrites, Brochant, II, 127). The first, you know, has been classed in the zirconian genus; and Mr. Lampadius, of Freyberg, imagined he found in it zircon and potash. Mr. Gruner, of Hanover, and Mr. Trommsdorf, of Erfurt, who have analysed the Greenland garnet, assert, that they found zircon in this also. Neither of these fossils however afforded Mr. Klaproth any of this earth, the following being the results of his analyses.

Analysis of some fossils.

Kannelstein.

The kannelstein yielded -		
	silex - -	38·80
	lime - -	31·25
	alumine - -	21·20
	oxide of iron	6·50
		97·75

* Annales de Chimie, vol. LXV, p. 185.

The

Greenland garnet.

The Greenland garnet

silex	43
alumine	16·50
magnesia	8·50
lime	1·75
oxide of iron	29·50
oxide of manganese	0·50
	98·75

Capillary pyrites native nickel.

The capillary pyrites is not a sulphuret of iron, but native nickel, with a little cobalt and arsenic.

Succinic acid separates iron from manganese.

Several years ago, you know, in my examination of amber, and its acid, which I have not yet published, I found, that the succinate of iron was insoluble in water; that of manganese, on the contrary, very soluble; and that on this I founded an easy method of separating these two metals, which has since been much employed by Klaproth, Vauquelin, and others. The basis of this process being the different, or I may say, inverse solubility of two saline combinations, it was obvious, that other acids might produce a similar result. Accordingly Mr. Berzelius mentioned the benzoic acid, which however I cannot think very suitable, as Mr. Trommsdorff asserts, that the benzoate of iron is very soluble. Mr. John has lately employed with the same view the oxalic acid, and acidulous oxalate of potash; and Mr. Simon, an eminent chemist of Berlin, confirms their utility in a paper he has just written on the analysis of some fossils, the colophonite, scapolite, &c.; and he observes they are preferable to the succinic acid, because the oxalate of iron is less bulky than the succinate

Benzoic acid supposed to do the same

Oxalic acid preferable

Titanium precipitated by tannin, not by gallic acid.

The same chemist has examined the phenomena exhibited by the gallic acid and tannin with titanium. This metal is not precipitated from its solutions except by tannin, or substances containing it.

I N D E X.

A.

B.

INDEX.

Continho,

Paroletti,

Smeaton,

INDEX.

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END OF THE TWENTY-FIFTH VOLUME.

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